Four Essays on Inequality and Migration

Inauguraldissertation

zur

Erlangung des Doktorgrades

der

Wirtschafts- und Sozialwissenschaftlichen Fakultät

der

Universität zu Köln

2017

vorgelegt

von

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aus

Frankfurt am Main

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Tag der Promotion: 14. Dezember 2017

Acknowledgments

Four years of academic work. During these I did not only learn a lot about economics, but rather more about myself. As the greatest challenge for me was not theory or exams, but the time I spent alone in front of a desk. Without the continuous support of my colleagues, friends, family, and supervisors, I would hardly had survived these often difficult periods. First of all, I thank David Jaeger, my supervisor, for taking me as a Ph.D. student and for constant guidance and counseling. Especially the research visit in New York at NBER has proven to be a fruitful period. I also thank my second supervisor, Michael Krause, for accepting me as a Ph.D. student and accepting my thesis. I thank Andreas Peichl, who took me to Mannheim and put me on the track. Special thanks to him also as a coauthor and executive supervisor. Next to him, I also thank my colleagues and fellow Ph.D. students at ZEW for sharing the experience of academic research. Sharing an office with Carina Neisser made writing and coding a lot easier. Moreover, working, darts, and drinks with Christian Wittneben, Maximilian Blömer, Paul Hufe, Florian Buhlmann, Max Löffler, and Carla Krolage has always been a pleasure and source of motivation. I thank Holger Stichnoth as my head of department during my single author paper and final phase of Ph.D.. I thank Clemens Fuest for two years of insights to public policy and leading a research center. I also thank Stephan Reichert and Thomas Kohl for showing me the engine room of a research center. I thank Martin Biewen as a coauthor and for guidance though out the early time of my doctoral studies. I thank Markus Gehrsitz, my coauthor and fellow Ph.D. student from David Jaeger for our joint projects and the time we spent together in New York and Glasgow. As I have been commuting between Frankfurt and Mannheim for some time, I thank my fellow train riders, especially Philipp Dörrenberg, Dominik Rehse, Steffen Viete, Sebastian Blesse, Andre Nolte, and Zareh Asatryan, for good times in the bistro and around the train station. I am thankful to my parents and family for the continuous supply of moral and food support. Finally, I thank Neri, who stayed by my side during these sometimes difficult times. I am happy we found each other in the woods. And our dog Poike, who provided constant distraction from otherwise endless writing and coding sessions.

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1 Introduction

1.1 Research Agenda

The book "Capital in the 21. Century" by Piketty (2014) marked the peak of attention for the topic of income inequality and spread awareness on the importance of ongoing research. During my master thesis, I had worked on decomposition methods for labor earnings. Motivated my this development in the literature and first experiences in academic research, I started working on income inequality as a topic for a Ph.D. thesis. The first project developed directly from the question how income inequality in Germany had developed over the last decade. Since 2005, we observed a growing labor market and economic recovery from the recession in the early 2000s. Given these increasing employment opportunities, we analyzed how income inequality developed. Our main finding was that income inequality did not increase further after 2005. On the one hand, within-year employment opportunities compensated otherwise rising inequality in annual labor incomes. On the other hand, income inequality did not decrease at all because all parts of the income distribution benefited from the economic boom after 2006.

The concept of equality of opportunity (EOP) is directly related to income inequality and labor market development. A natural question is the dependence of inequality in adult outcomes on family background and childhood circumstances when working on income inequality and its underlying causes. The literature on EOP divides all potential determinants of adult outcomes in two main categories, circumstances and effort. The former comprise of all factors outside the realm of personal responsibility for which society should not hold people responsible, such as parental education. The latter represents personal choices and effort independent of circumstances. Total equality of opportunity would exist if inequality in adult income depended on effort. As I was interested in income inequality in the household context as well, the second project stemmed from the question how measurement of EOP depends on the partner in a relationship. We found that taking into account the spouse in measuring EOP matters empirically and from a theoretical perspective.

A prominent circumstance in the EOP literature is the country of origin. The increasing refugee migration all over Europe and Germany in particular raised the question which opportunities these newly arriving immigrants would face. Working on such timely events has benefits and disadvantages. On the one hand, one is sure to work on the frontier of applied research and social debate. On the other hand, data is scarce and the consequences of events cannot fully be observed immediately. Hence, we started to work on the short-terms effect of increased refugee migration to Germany in terms of employment, crime, and voting behavior. Our analysis indicates a small effect on crime rates, mostly around reception centers and with respect to drug crimes. The results on labor market integration showed an increase in foreign job seekers.

The recognition of refugee status is the necessary premise for labor market integration, but not necessarily sufficient. Extensive research has been conducted on the integration of refugees into the labor market and society. However, no consensus has been reached which factors perform best at facilitating job search and employment prospects. Many governments around de world use allocation schemes in order to prevent the formation of enclaves. In 2016, also the German government implemented a residency obligation for accepted refugees. The last chapter of my Ph.D. thesis is about the effectiveness of such an allocation scheme in fostering labor market integration and preventing criminal behavior of accepted refugees. Thereby, I combine insights from previous work in the final chapter. So far, I find a small but significantly positive effect of a residency obligation on employment prospects.

1.2 Abstracts

Chapter 1: Why did income inequality in Germany not increase further after 2005? While income inequality in Germany considerably increased in the years before 2005, this trend was stopped after 2005. We address the question of what factors were responsible for the break in the inequality trend after 2005. Our analysis suggests that income inequality in Germany did not continue to rise after 2005 for the following reasons. First, we observe that the general rise in wage inequality that explained a lot of the inequality increase before 2005, became less steep (but did not stop) after 2005. Second, despite further increases in wage inequality after 2005, inequality in annual labor incomes did not increase further after 2005 because increased within-year employment opportunities compensated otherwise rising inequality in annual labor incomes. Third, income inequality did not fall in a more marked way after 2005 because also the middle and the upper part of the distribution benefited from the employment boom after 2006. Finally, we provide evidence that the effect of a wide range of other factors that are often suspected to have influenced the distribution such as capital incomes, household structures, population aging, changes in the tax and transfer system, and the financial crisis of 2008 did not significantly alter the distribution after 2005.

Chapter 2: Accounting for the spouse when measuring inequality of opportunity. The existing literature on inequality of opportunity (IOp) has not addressed the question of how the circumstances and choices of spouses in a couple should be treated. By omitting information relevant to the spouse in IOp estimations, the implicit assumption has been full responsibility for the spouse's income, effort and circumstance variables. In this paper, we discuss whether or not the spouse's characteristics should be treated as responsibility factors. Using German micro data, we analyze empirically, how IOp estimates are affected when a spouse's circumstance or effort variables are included in the analysis. We find that including spousal variables can increase IOp measures by more than 20 (35) percent for gross (net) earnings. The less responsibility assumed for the partner's variables, the higher the IOp estimate.

Chapter 3: Jobs, Crime, and Vote - a short-run analysis of the German refugee crisis. Millions of refugees made their way to Europe between 2014 and 2015, with over one million arriving in Germany alone. Yet, little is known about the impact of this inflow on labor markets, crime, and voting behavior. This article uses administrative data on refugee allocation and provides an evaluation of the short-run consequences of the refugee inflow. Our identification strategy exploits that a scramble for accommodation determined the assignment of refugees to German counties resulting in exogenous variations in the number of refugees per county within and across states. Our estimates suggest that migrants have not displaced native workers but have themselves struggled to find gainful employment. We find small increases in crime in particular with respect to drug offenses and fare-dodging. Our analysis further suggests that counties which experience a larger influx see neither more nor less support for the main anti-immigrant party than counties which experience small migrant inflows.

Chapter 4: Integrating Refugees: How Effective is a Residency Obligation? The effects of increased migration on living conditions in the hosting country and the challenge of integration have been widely discussed in policy and academic literature. In order to manage the flow of over 900,000 refugees who came to Germany in 2015 and to prevent clustering of migrants, the German government put in place a residency law for accepted migrants starting in 2016. I make use of the fact that some of the German states implemented a residency obligation, while others did not, and analyze its effect on employment of accepted refugees and individuals under subsidiary protection. I find that the residency obligation increases labor market integration. However, I do not find significant results regarding the effect of migration on crime.

1.3 Structure and Coauthors

The ordering of topics in this Ph.D. thesis is supposed to reflect two aspects of my doctoral studies. First, it is ordered chronologically, with early projects as the first chapters. Second, I intend to provide a representative overview on my work during the last four years. While I have worked on other topics and published further paper, the included chapters represent the main topics of interest from my Ph.D. years.

With respect to my coauthors, all project members participated equally in each paper. During the first two projects, the senior coauthors, Martin Biewen and Andreas Peichl, provided useful guidance in structuring a research project. Within the third project together with Markus Gehrsitz, who was also a Ph.D. student of David Jaeger at the starting point of our idea, we jointly developed the research idea and shared all steps of data acquisition, analysis, and writing equally.

2 Why did income inequality in Germany not increase further after 2005?

2.1 Introduction

Despite considerable public interest in distributional issues in Germany as well as in many other countries, systematic analyses of the evolution of the income distribution and its potential determinants remain surprisingly rare. There is a well-established literature in labor economics that studies rising inequalities in wage incomes (for Germany, see Dustmann et al. (2009), Fuchs-Schündeln et al. (2010), Fitzenberger (2012), and Card et al. (2013), Dustmann et al. (2014), Biewen and Seckler (2017), among others). The distribution of wages paid in the labor market is certainly a major component of the overall distribution of incomes, and it is important for our understanding of how labor markets work. However, the final distribution of disposable incomes in a population is the complex outcome of a large number of further factors such as the development of household forms, the employment opportunities and employment decisions of households, the influence of other income sources such as capital incomes, and the transformation of market incomes into net disposable incomes through the tax and transfer system.

As documented in a number of previous studies (Biewen and Juhasz (2012), Grabka et al. (2012), Grabka and Goebel (2013), IAW (2013), Schmid and Stein (2013), Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2011, 2015), Feld and Schmidt (2016), Battisti et al. (2016)), there was a considerable increase in income inequality in Germany during the period 1999 to 2005, which however did not continue after 2005. As shown in previous contributions (Biewen and Juhasz (2012), and IAW (2013)), the inequality rise before 2005 was mainly related to increases in wage inequality conditional on employment, to changes in the level and structure of household employment outcomes, and to tax reforms. The goal of this paper is to investigate why the rise in income inequality stopped after 2005. To this end, we provide a careful analysis of the influence of a wide range of factors on the distribution after 2005, and we determine how the influence of the factors that were identified as important for the development before 2005 changed after 2005. Among other things, we provide evidence that reconciles seemingly contradictory results on further increasing wage inequality derived from administrative data sources such as the SIAB and the finding that inequality in yearly labor incomes as measured in a household survey like the SOEP did not increase further after 2005. We also address the puzzling question why the dynamic development of the German labor market after 2005 did not lead to a more marked decrease in income inequality. Finally, as the period we study covers the financial crisis of 2007/2008 and the subsequent great recession, we are able to assess the influence of these important global events on the German distribution.

Our study complements and extends the limited number of contributions that deal with the general evolution of the German income distribution, see Grabka et al. (2012), Grabka and Goebel (2013), IAW (2013), Schmid and Stein (2013), Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2011, 2015), Feld and Schmidt (2016), and Battisti et al. (2016). Grabka et al. (2012), Grabka and Goebel (2013),Schmid and Stein (2013), Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2011, 2015), Feld and Schmidt (2016) and Battisti et al. (2016) document and discuss a number of relevant trends in the distribution of equivalized net incomes but do not explicitly model their potential influence on the distribution. There are only a few contributions that adopt a more analytic approach to changes in the German income distribution, see in particular Peichl et al. (2012) and Jessen (2016). For a comprehensive overview of a number of relevant developments in the German income distribution, see Corneo (2015).

The rest of this paper is structured as follows. Section 2 introduces the data on which our study is based. In section 3, we present general trends as well as our analysis of the potential influence of different factors on the distribution. Section 4 concludes. The appendix describes details of the different techniques used by us to estimate the effect of underlying factors on the distribution.

2.2 Data

Our study is based on data from the German Socio-Economic Panel (SOEP).[1] Despite a number of limitations, the SOEP is the only data suitable for the present analysis, as it is the only data set that provides sufficiently rich information on annual incomes and its different sources along with a range of individual and household characteristics. Our analysis of the influence of different factors on the income distribution focuses on the period 2005 and 2011, but in many cases we show long-term trends from 1994 onwards. Our dependent variable is real annual equivalized net income in prices of 2011 which is calculated from annual net household income. The latter is provided in the SOEP as annual household gross income (reported by all household members) minus household income taxes and household social security contributions (both calculated by the data provider DIW) plus household public transfers (as reported by the household members). Household gross income comprises all private sources of income including

[1] Wagner et al. (2007).

labor income, capital income (i.e. income from interest, dividends, rent etc. but not including capital gains), private transfers and private retirement incomes.

Following common practice, we include in household gross income imputed social security contributions for civil servants (which are fully paid by the employer and thus do not appear in civil servants' reported gross income) as well as imputed rental values for owner occupied housing. The latter are calculated by the data provider as the rent for owner occupied housing minus operating costs, interest payments on mortgages and property taxes. Imputed rental values are the equivalent of income from rent received by owners who do not use their property as their own housing (they are thus a form of capital income). Household public transfers include the full range of government transfers such as unemployment benefits, child benefits, student grants, subsistence allowance as well as old age pensions from the public pension system. As completed annual incomes can only be reported for the previous year, we shift all income information by one year. In this way we make sure that our analysis of a particular year refers to the incomes reported by the population for that year.

We equivalize household annual net incomes using the widely used modified OECD equivalence scale, where the household head receives a weight of one, further household members over 14 years receive a weight of .5 and those aged 14 years or less a weight of .3. Note that our analysis refers to individuals not to households. This means that all individuals in the household (including children) are assigned the value of equivalized personal income $hhnet/s$ (where $hhnet$ is the household net income and s is the sum of equivalence weights for the household). We distinguish between six different household types: (i) single pensioner households (65 years or older), (ii) multiple pensioner households (at least one household member 65 years or older and no household member under 55 years), (iii) single adults without children, (iv) multiple adults without children, (v) single adults with children, and (vi) multiple adults with children.

We make use of a wide range of further household characteristics.[2] In particular, we consider the number of adults in the household, the proportion of female adults in the household, the proportion of adult household members with different educational qualifications (university degree, high school and/or vocational training, no such degree or qualification), the proportion of household members with disabilities, the proportion of married adults in the household, the proportion of household members in different age groups (0-3 years, 4-11 years, 12-17 years, 18-30 years, 51-64 years, 65 years or older) as well as a variable indicating whether the household lived in East Germany. In order to describe household employment, we define the following ordinal range of household employment outcomes: (i) no part-time or full-time job in the household, (ii) no full-time job but at least one part-time job in the household, (iii) one full-time

[2]For some summary statistics, see table 2.3.

job but no part-time job, (iv) one full-time job and at least one part-time job, and (v) at least two full-time jobs in the household. Our definition of part-time jobs also includes marginal employment ('Mini-/Midijobs'). Category (iii) thus also covers the case where one and the same individual holds both a part-time job (e.g. 'Minijob') and a full-time job. Note that all the individual and household characteristics described above refer to what households reported about themselves at the time the survey was carried out.

2.3 Overall trends

Figures 2.1a and 2.1b display general inequality trends in equivalent incomes over the period 1994 to 2011 measured by a number of commonly used indices.[3] There are three distinctive sub-periods: (i) 1994 to 1998: slightly decreasing inequality and poverty, (ii) 1999 to 2005: substantially rising inequality and poverty, and (iii) 2006 to 2011: constant or slowly declining inequality along with constant or slightly increasing poverty. The period 1999 to 2005 was analyzed in detail in Biewen and Juhasz (2012) and IAW (2013). In this paper, we focus on the period 2006 to 2011 in order to see to how the influence of the factors that were identified as important for the inequality increase between 1999 and 2005 changed after 2005.

— Figures 2.1a and 2.1b around here —

The evolution of mean and median equivalized incomes over the period 1994 to 2011 are shown in figure 2.2a. Again, three different subperiods can be distinguished: (i) substantially rising average equivalent incomes between 1994 and 1998, (ii) stagnating or falling incomes between 1999 and 2005, and (iii) moderately increasing incomes between 2006 and 2011.

— Figure 2.2a around here —

The fact that there was a clear rise in median equivalent income from 2006 to 2011 raises the question to what extent the rise in the poverty rate over the same period was due to a rise in the relative poverty line of 60 percent of the median rather than due to falling incomes of the poor. Figure 2.2b shows that this was completely the case. If the poverty line had been fixed at its 2006 level, the proportion of the poor would even have declined over the period 2006 to 2011. This means that low income groups

[3]For these indices, see, e.g., Cowell (2000). Following common practice in the European Union, we define the poverty rate as the proportion of individuals with incomes less than 60% of the median. 'Income richness' is defined as the proportion of individuals whose incomes are higher than two times the median.

did not participate in income increases to the same extent as more affluent groups but also that low income groups did not suffer absolute income losses over this period.

— Figure 2.2b around here —

Table 2.1 summarizes the changes in the distributional indices over the period 2005 to 2011 along with their statistical significance. In order to increase statistical precision and in order to make the analysis less dependent on individual years, we consider the changes between the pooled observations 2005/2006 and 2010/2011.[4] It turns out that the measured changes were economically small and in many cases statistically insignificant. For example, a change of the Gini by -.005 corresponds to a redistribution of one percent of mean income from each individual in the upper half of the distribution to each individual in the lower half of the distribution (Blackburn, 1989). The change in average income was also moderate (plus 685 Euros for the mean and plus 744 Euros for the median), amounting to an increase of the average standard of living over the period considered by us of around 3 to 4 percent.

— Table 2.1 around here —

Figure 2.3 illustrates the overall change of the distribution of equivalent incomes between 2005/2006 and 2010/2011.[5] The figure confirms the trends described previously, i.e. there was a significant shift of the distribution to the right, while its dispersion stayed constant or was slightly narrowing.

2.4 Empirical analysis

The goal of the present analysis is to understand why the rise in income inequality that took place between 1999 and 2005 did not continue after 2005. The evidence in Biewen and Juhasz (2012) and IAW (2013) suggests that around 40 to 50 percent of the rise in inequality between 1999 and 2005 can be accounted for by changes in labor market returns (i.e. labor incomes given household characteristics and household employment outcomes), around 20 to 30 percent by changes in the level and structure of household employment outcomes, and another 20 to 30 percent by a series of tax reforms implemented between 1999 and 2005 (which were regressive in the sense that they benefited top incomes more than middle and low incomes). The direct distributional effects of

[4]We compute bootstrap confidence intervals taking account of the longitudinal correlation in our data set and the clustering of observations at the household level (Biewen, 2002).

[5]In order to facilitate graphical analysis, the figure shows the distribution of *log* equivalent incomes.

changes in the transfer system (in particular the Hartz IV reforms), changes in household structures as well as changes in the composition of the population with respect to age, education, nationality and other characteristics appear to have been very small.[6]

The apparent stability of the income distribution after 2005 is puzzling given the dynamic development of the labor market during that period and given the evidence in studies on wage inequality which generally report further inequality increases after 2005 (Card et al. (2013), Dustmann et al. (2014)). However, the apparent stability of the overall distribution may well be the result of countervailing trends which partly or fully offset each other. For example, it may be the case that increases in income inequality due to further rising wage inequality were fully compensated by inequality decreasing employment changes, or that the effects of each of these factors were counteracted by other factors that are often thought to influence the distribution such as capital incomes, population aging, changes in household forms, or changes in the tax system.

In the following, we provide a careful analysis of the effects of a wide range of such factors on the distribution after 2005 using a comparable framework as in Biewen and Juhasz (2012). This allows us to determine to what extent the influence of factors that were identified as important for the evolution between 1999 to 2005 changed after 2005. We do this by employing different techniques of constructing counterfactual distributions in which we change a given factor in isolation and consider its effect on the distribution of equivalized incomes. Detailed descriptions of the methods used for each factor are provided in the appendix.

2.4.1 Changes in household structures

Changes in the composition of the population with respect to different household types will change the distribution of incomes if average incomes differ systematically across household types. For example, inequality and poverty may rise if household types with unfavorable income positions (e.g. lone parents) become more prevalent in the population. In the present analysis we distinguish between the six household types defined in section 2.2. Figure 2.4a shows the evolution of their population shares over time.

— Figure 2.4a around here —

During the period under consideration (shaded gray), we observe a marked increase in the population share of multiple adult households without children at the cost of

[6] As pointed out in Biewen and Juhasz (2012) and IAW (2013), these analyses focus on the *direct* ('first-round') effects of the respective factors on the distribution. There may well be more indirect effects, e.g. the changes in the transfer system may lead to employment increases. In our analysis, such indirect effects will be included in the effect of employment changes on the distribution.

multiple adult households with children. We also observe slight increases in the proportion of single adult households without children and that of multiple pensioner households. In order to gouge the potential effect of these non-negligible changes on the distribution of equivalent incomes, we construct an income distribution that would result if we keep everything at the level of the base period 2005/2006 but change the composition of the population with respect to household types to its 2010/2011 level.[7]

— Figure 2.4b and table 2.2 around here —

The result of this exercise is shown in figure 2.4b. The effect on our range of distributional indices is given in table 2.2. Figure 2.4b shows the overall change of the distribution between 2005/2006 and 2010/2011 (bold line, also shown as the dotted line in figure 2.3) along with distributional change induced by only changing household structures (dashed line in figure 2.4b). The observed changes in the distribution of household types leads to a small shift of the upper half of the distribution to the right. This makes sense as we observe increasing population shares of household types with higher average equivalent incomes at the cost of those with lower average equivalent incomes. However, the total effect of these changes on inequality is rather small and in most cases statistically insignificant (see table 2.2).

2.4.2 Changes in other household characteristics

Next, we consider the effect of changes in further household characteristics. In particular, we consider changes in the composition with respect to nationality (e.g. as the result of migration), gender, disabilities, educational qualifications, the more detailed age structure of households, and residence in East vs. in West Germany. As table 2.3 shows, most of these household variables did not change substantially between 2005/2006 and 2010/2011. The only exceptions were slight changes in the share of certain age groups (higher shares of older household members at the cost of lower shares of younger household members) and a slight shift towards higher educational qualifications.

— Table 2.3 around here —

In order to gouge the effect of these changes on the distribution of equivalent incomes we construct a distribution that would result if we keep everything as in 2005/2006 but change the joint distribution of the list of household characteristics shown in table 2.3 to the level of 2010/2011.[8] The results of doing this are shown in figure 2.4b and

[7]See appendix for a more detailed description.
[8]See appendix for more details.

table 2.4. The changes in question appear to induce a slight shift of the middle and the upper part of the distribution to the right which is consistent with the observation that there were more households with higher educational qualifications and fewer children in 2010/2011 when compared to 2005/2006. Although table 2.4 shows that these effects were often statistically significant, they were small in economic terms. For example, the induced change for the poverty rate is around one third of a percentage point, the effect for the Gini and other inequality indices was similarly small.[9]

— Table 2.4 around here —

2.4.3 Changes in household employment outcomes

During the period under consideration, the German labor market experienced considerable employment gains. Employment began to rise after 2006 leading to record levels and to the lowest unemployment rate since reunification (Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung (2011), chapter 7). This positive trend was not stopped by the financial crisis of 2007/2008, during which the German labor market stayed surprisingly robust (Möller (2010)). In order to see how these trends manifested themselves at the household level, we distinguish between the five different household employment intensities described in section 2.2. The evolution of the proportion of households in each of the five groups are shown in figure 2.5a. The decline in unemployment is reflected in the proportion of households in which no household member is employed (see the line with '0 PT, 0 FT' in figure 2.5a). The proportion of such individuals continuously rose between 1999 and 2005, in line with the general rise in the unemployment during this period. After 2007, the fraction of individuals in households without employment began to fall again, returning to the low level of 1999 by the year 2010. We also observe in 2005 a significant break in the trend of the proportion of individuals in households with at least two full-time workers, which began increase from 2005 onwards, after a long period of decline. Finally, figure 2.5a shows a strong increase in the number of households with part-time but no full-time employment and a secular decline of households with exactly one full-time job but no part-time (or marginal) employment. Note that all of these figures are based on household employment at survey time. This information enables us to analyze the effect of general trends in the level and the structure of employment across years, but not the effect of within-year employment variations. We address the potential effect of such within-year employment variations further below.

— Figure 2.5a around here —

[9]Note that, apart from statistical significance, the relatively small changes observed may well lie within the margin of potential misspecification of our counterfactual distributions.

In order to assess the effects of these changes in the structure of household employment outcomes on the distribution of equivalized net incomes, we construct a counterfactual distribution in which we keep everything as in 2005/2006 but shift to the level of 2010/2011 the probabilities that a household with the list of characteristics shown in table 2.3 reported one of the five employment outcomes shown in figure 2.5a.[10] In this way, we capture the effect of changes both in the general level and in the composition of employment between 2005/2006 and 2010/2011, conditional on household characteristics. Perhaps surprisingly, the result of this operation as shown in figure 2.5b (dashed line) and table 2.5 suggests that the employment changes between 2005/2006 and 2010/2011 did not lead to major changes in the distribution of equivalent incomes. There was a slight shift of the distribution to the right, but there was no significant change in inequality. This is in stark contrast to the results in Biewen and Juhasz (2012) where the same counterfactual operation explained up to 30 percent of the considerable inequality rise between 1999/2000 and 2005/2006. It appears that, contrary to the employment changes between 1999/2000 and 2005/2006, the employment changes between 2005/2006 and 2010/2011 affected households more proportionally.

— Figure 2.5b and table 2.5 around here —

In order to investigate why the changes in household employment levels and composition between 2005/2006 and 2010/2011 did not yield more noticeable changes in the distribution, table 2.6 describes which kind of households benefited from rising employment. The table describes how households with certain employment outcomes in a given year changed their employment to the next year and averages these yearly changes over the years 2005 to 2011. For example, the average yearly employment gains (full-time or part-time) between 2005 and 2011 in households with exactly one full-time job was .123, while that of households without employment (e.g. due to unemployment) was only .085. This means that the employment boom did not only bring unemployed households into employment but also increased employment in households in which there already was full-time employment.[11]

— Table 2.6 around here —

[10] See appendix for more details.

[11] Note that the net employment changes reported in the table mask a great deal of heterogeneity, as households change their employment behavior for many different reasons such as demographic or labor market events, independently of aggregate employment gains. Given that we consider year-to-year differences, the numbers in the table control for household characteristics that are constant from one year to the other. In view of the highly unbalanced nature of the SOEP, we do not restrict our sample to households that are present in all years, i.e. we do not consider longer-term fixed effects. The negative net employment changes in households with at least two full-time jobs are the result of a ceiling effect (being the top category, these households are likely to downgrade their employment).

A more detailed analysis distinguishing between full-time and part-time employment gains reveals that especially households who already reported part-time employment but also households with full-time employment were able to further increase their full-time employment. In particular, the middle panel of table 2.6 suggests that households with one full-time and at least one part-time job upgraded to two or more full-time jobs. As such households tended to belong to the middle or even the upper part of the distribution, these employment gains were not necessarily inequality reducing. The results in the lower panel of table 2.6 point in the same direction. Additional part-time employment was not only picked up in households without any employment but especially in households in which there was already full-time employment (or even double full-time employment). We observe an average decrease of part-time employment in households with part-time (but no full-time) jobs, suggesting that these households were able to upgrade their part-time employment to full-time employment (consistent with the numbers in the middle panel of table 2.6).

— Figure 2.6 around here —

The hypothesis that the expansion of employment from 2006 to 2011 not only benefited workless households but also households that already had employment is given further support in figure 2.6. The figure compares the average number of jobs per household in non-pensioner households across the deciles of the distribution of equivalent incomes. It turns out that the average number of jobs per household in 2010/2011 lies above the average number of jobs in 2005/2006, uniformly across the deciles of the income distribution. Distinguishing between full-time and part-time jobs, the interesting finding emerges that the average number of part-time jobs was higher in 2010/2011 at the bottom and the middle of the distribution, whereas the higher number of full-time jobs applied to the middle and to the upper part of the distribution. This helps to understand why employment gains did not lead to a more marked decrease in inequality after 2005 as not only poor but also better-off households participated in the employment boom.

2.4.4 Changes in labor market incomes

As the next factor, we consider inequality in household labor incomes. Changes in the labor market returns of individual characteristics such as educational qualifications or work experience have been the subject of an important recent literature in labor economics (Dustmann et al. (2009), Fuchs-Schündeln et al. (2010), Fitzenberger (2012) and Card et al. (2013)). As shown in Biewen and Juhasz (2012), such changes were the most important factor behind the evolution before 2005, accounting for up to 40 to 50 percent of the inequality increase in net equivalent incomes between 1999 and 2005. In order to investigate the role such changes for the evolution of net income inequality after 2005, we show in figure 2.7a the evolution of inequality in equivalized gross household labor market income including households with zero labour market incomes (i.e. households without employment). The graph suggests growing inequality in gross household labor incomes up to 2005, followed by break in the trend and slightly declining inequality after 2005. The slightly declining trend after 2005 might be due the fact that some households increased their labor income from previously zero to a positive amount (as a consequence of the employment boom).[12] By contrast, figure 2.7b shows the evolution of inequality in household labor incomes *conditional on employment* (i.e. excluding households with zero labor income). The resulting inequality trend after 2005 is now essentially flat, implying that the slight inequality decline in figure 2.7a was indeed caused by falling numbers of households with zero labor income.

— Figures 2.7a and 2.7b around here —

Figure 2.7b suggests that the strong effect of increasing wage inequality on overall income inequality that identified in Biewen and Juhasz (2012) for the period 1999 to 2005 did not continue after 2005. In order to measure the impact of changes in labor market returns to household characteristics and household employment outcomes on overall income inequality, we construct a counterfactual distribution of equivalent incomes that results if we keep everything as in 2005/2006 but shift the labour market returns to household characteristics and household employment outcomes to the level of 2010/2011. These returns are estimated by regressions of annual household labor income on household characteristics and household employment constellations as defined in sections 2.4.2 and 2.4.3, separately by household type (see appendix for more details). They reflect the labor market remuneration of households conditional on their characteristics and employment status. The resulting effects are shown in figure 2.5b and in table 2.7. It turns out that rising labor market returns between 2005/2006 and 2010/2011 explain very well the overall shift of the distribution of equivalized incomes to the right, but the numbers in table 2.7 suggest that this did not significantly change income inequality.

— Table 2.7 around here —

At first sight, the finding in table 2.7 that inequality in labour incomes conditional on household employment outcomes did not increase anymore after 2006 appears to

[12]The graph shows only the Gini as other commonly used inequality indices cannot deal with zero values.

contradict evidence based on other data sources such as the SIAB (Sample of Integrated Labor Market Biographies) or the VSE (Structure of Earnings Survey), see Fitzenberger (2012), Card et al. (2013) and Moeller (2016). For example, based on the SIAB, Moeller (2016) finds that wage inequality increased up to 2011, but presents evidence that there was no further increase after 2011. However, there are important differences between these studies and the analysis of labor incomes conducted here. First, the studies cited focus on individual wage income from dependent full-time employment, whereas our data defines labor incomes in a more comprehensive way including part-time employment, marginal employment and self-employment. For example, studies based on IAB data typically only cover prime-age full-time employment excluding the self-employed, civil servants, and apprentices. Second, we consider household labor income as opposed to individual wage income, which we equivalize (in order to make the link to equivalized net income, our income concept of interest). Third, and most importantly, the studies cited usually consider inequality in monthly, daily or hourly wages, whereas we focus on the annual labor incomes of all household members.

— Figures 2.8a and 2.8b around here —

In order to shed more light on these differences, we provide in figures 2.8a and 2.8b the development of inequality in *individual monthly* labor income (individual annual labor incomes divided by the number of months worked) restricted to a group of individuals that resembles most closely the definition of prime-age full-time employment excluding the self-employed, civil servants and apprentices that is considered in papers such as Card et al. (2013), Dustmann et al. (2014) or Moeller (2016). As figure 2.8a shows, inequality in this wage measure also follows a long-term upward trend beyond the year 2005, although there are considerable fluctuations around this trend, probably due to the moderate number of observations in the SOEP. There also appears to be a flattening of the upward trend after 2005, although it is hard to tell whether this flattening is merely a consequence of the fluctuations in the time series.[13] Figure 2.8b considers the same population subgroup but now also includes the cases of part-time and marginal employment (but still excluding the self-employed, civil servants and apprentices). The picture is similar to the previous one, featuring a general upward trend with a temporary dip after 2006.

— Figure 2.9a and 2.9b around here —

In figure 2.9a, we still focus on *individual monthly* wages but switch to the population considered in our main analysis, i.e. all individual wage incomes including the self-employed, civil servants, apprentices, and including full-time, part-time and marginal

[13]A flattening of the increase in wage inequality can also be observed in Moeller (2016), but there the flattening sets in after 2007.

employment. As in the cases before, there is a general upward trend extending beyond 2005, but the trend becomes less steep after 2005. Figure 2.9b makes another step towards the wage income measure used in our analysis by considering inequality in *individual annual* wage income covering all types of employment. The figure shows that this transition from individual *monthly* to individual *yearly* wage income is the decisive one to eliminate the upward trend in wage inequality after 2005. The difference between individual monthly and individual yearly wage incomes must be the result of increased possibilities to prop up labor incomes by occasional marginal employment ('minijobs') or by the fact that falling unemployment and increasing employment implied fewer (or shorter) interruptions of annual employment. This conjecture is confirmed in figures 2.10a and 2.10b which show the evolution of the average number of months worked per year for working-age individuals along with its coefficient of variation. These series also show a break in the trend around the year 2005. After a long period of decline, the average number of full-time months per individual started to increase again after 2006. Together with the secular upward trend in the number of part-time months, this resulted in a clear upward trend in the total number of months worked from 2005 onwards. As shown in figure 2.10b, this also resulted in declining inequality in months worked per year across the working-age population after 2005, i.e. the employment boom led to a more equal distribution of within-year employment.

We therefore conclude that there are two reasons for the break in the inequality trend in household annual labor incomes after 2005 (figure 2.7b). First, the trend of rising inequality in *individual monthly* labor incomes became less steep (but did not stop) after 2005 (figure 2.9a). And second, despite further increases in monthly wage inequality after 2005, inequality in *annual* labor incomes did not increase further because increased within-year employment opportunities compensated otherwise rising inequality in annual labor incomes (figures 2.9b to 2.10b).

2.4.5 Changes in capital incomes

Apart from labor incomes, changes in household capital incomes as well as changes in the ratio of capital to labor incomes may influence the distribution of equivalized net incomes. Such an effect is often conjectured but we do not know of much research about this. Potential effects of this kind are particularly interesting as capital incomes may have been substantially affected by the financial crisis of 2007/2008.[14] Figure 2.11a displays the evolution of inequality in equivalized household capital incomes including households with zero capital income. Note the very high level of the Gini of over

[14]The potential impact of the financial crisis 2007/2008 on different components of the distribution is discussed in more detail in section 2.4.8.

80 percent which is due to the fact that most households have zero capital incomes. Inequality in equivalized capital incomes decreased after 2005 - and in an accelerated way after the financial crisis 2007/2008 - but returned to the level of 2005 in the year 2011. A potential reason for the falling inequality in household capital incomes after 2007 appears to be that average capital incomes (measured in households with positive capital incomes) fell from 2007 onwards after they had been continuously increasing since at least 1994 (figure 2.11b).

— Figures 2.11a and 2.11b around here —

We assess the potential influence of these changes on the distribution of net equivalized incomes by constructing a distribution that results if everything is kept at the level of 2005/2006 but the distribution of capital income is shifted to its level of 2010/2011. We do this by assuming that the ranks of households in the distribution of capital incomes stay constant, i.e. a household who was at the 36th percentile of the capital income distribution in 2005/2006 is assigned the 36th percentile of the capital income distribution of 2010/2011.[15] As figure 2.5b and table 2.8 show, the effect of doing this is essentially zero. Apart from the very modest changes in the distribution of capital incomes, the likely reason for this is that capital incomes represent only a small share in overall household gross income in our data (seven to nine percent), as shown in figure 2.12.

— Table 2.8 around here —

Our analysis of capital incomes is subject to the important caveat that very high incomes are likely to be underrepresented in sample surveys like the SOEP. As capital incomes are mostly concentrated at the top of the distribution, our analysis may therefore underestimate the influence of this factor on the distribution of net equivalized incomes. For a detailed discussion of this issue, see Drechsel-Grau et al. (2015) and Bartels and Schröder (2016). Note however, that taking into account evidence from all available data sources, Germany generally does not seem to display the drastically rising shares of top incomes observed in some Anglo-Saxon countries (Jenderny and Bartels (2015), and Atkinson et al. (2011)).

— Figure 2.12 around here —

[15]See appendix for more details.

2.4.6 Changes in the transfers system

As shown in Biewen and Juhasz (2012), the fundamental changes to the transfer system introduced by the Hartz-IV reform in 2005 had surprisingly small direct effects on the distribution of net incomes in Germany. The main reason for this seems to be that transfer incomes still generally represent a limited fraction of all incomes in the population, and that there were not only losers but also a considerable number of winners of these reforms.[16] After the fundamental changes of the Hartz-IV reform there were only relatively minor changes in the transfer system after 2005. These included, besides minor adjustments to different benefits and allowances, the (re-)extension of the maximal entitlement period of unemployment benefits I (ALG I) from 18 to 24 months for older workers in January 2008, as well as the abolition of a particular allowance ('Armutsgewöhnungszuschlag') in January 2011 that aimed to mitigate the transition from unemployment benefits I (depending on previous wages) to the generally much lower unemployment benefits II (a lump-sum and means-tested basic income).[17]

In order to assess the potential effect of these changes on the distribution of net equivalized incomes, we construct a counterfactual distribution in which we keep everything at the level of 2005/2006 but assign to each household the change in benefits that results from the changes in the transfer system. For the computation of the difference in transfers under the old transfer system of 2005/2006 and the new transfer system of 2010/2011, we use the microsimulation model $IZA\Psi MOD$.[18,19]

The results shown in figure 2.13 and table 2.9 are statistically significant but, again, economically small. There was a small shift from the very bottom of the distribution to the lower middle part which was the likely result of several adjustments of transfer payments to general income growth. Note that, as expected, the changes of the transfer system were concentrated in the lower part of the distribution as only these households

[16] For more details, see IAW (2013), p. 120, which also features a before/after comparison of the years 2003/2004 and 2007/2008. The Hartz-IV reforms led to additional fiscal spending of several billions of Euros flowing to the bottom of the income distribution, showing that the view that these reforms were inequality increasing is too simple.

[17] This allowance amounted to two thirds of the difference between unemployment benefit I and unemployment benefit II in the first year of unemployment benefits II, and to one third of the difference in the second year.

[18] See appendix and Löffler et al. (2014). We thank Nico Pestel and Eric Sommer for their help in using $IZA\Psi MOD$. The $IZA\Psi MOD$ version used by us covers all changes in transfers and allowances over the period under investigation including the abolition of the 'Armutsgewöhnungszuschlag' but excluding the extension of the maximal entitlement period for older workers. We do not expect the latter changes to have any noticeable effect on the distribution however, as the individuals affected represented a tiny proportion of the population and as the much more substantial changes in unemployment benefits due to the Hartz-IV reform did not have any noticeable direct effects on the overall income distribution (Biewen and Juhasz, 2012).

[19] Note that this analysis does not consider potential behavioral adjustments to the changes in the transfer system. However, given the evidence in Jessen (2016) such potential behavioral reactions appear to be rather small and would generally counteract the direct effects on incomes.

receive substantial amounts of transfer incomes. We point out that our analysis only captures the direct (i.e. mechanical) effects of the transfer changes on the income distribution, excluding the potential indirect effects these transfer changes may have on quantities such as employment. Such indirect effects will be included in our analysis of the effects of employment changes on the distribution (see above).

— Figure 2.13 and table 2.9 around here —

2.4.7 Changes in the tax system

Apart from minor adjustments to exemptions and allowances, there were two relatively substantial changes to the German tax system during the period considered by us. The first one was the introduction of a so-called 'rich tax' (January 2007) which raised the marginal tax rate from 42 to 45 percent for taxable incomes above 250.000 Euros. The second one was the introduction of a withholding tax ('Abgeltungssteuer') for certain kinds of capital incomes with a proportional rate of 25 percent which replaced the taxation of these capital incomes within the more general system of income taxation (January 2009).

In order to gauge the effect of these changes on the distribution of equivalized net incomes, we compute a counterfactual income distribution in which we keep everything as in the situation of 2005/2006 but apply to the taxable incomes of these years the changed tax system as of 2010/2011. We do this by replicating the information on household tax payments in the SOEP (which are based on tax simulations carried out by the data provider DIW) using flexible regression methods and by counterfactually applying the tax schedule obtained in this way to the taxable incomes in 2005/2006.[20] Figure 2.13 and table 2.10 suggest that these changes implied small changes to the upper part of the distribution of net equivalized incomes, which conforms to the expectation that the tax changes under consideration (the 'rich tax' and the withholding tax for capital incomes) mostly referred to this part of the distribution. As figure 2.13 shows, higher incomes appear to have been net beneficiaries of these changes, although the gains were small in economic terms. Again, the important caveat applies that top incomes are likely to be underrepresented in the SOEP so that we might underestimate the effects of the tax reforms considered.

— Table 2.10 around here —

[20] This method was used, e.g., by Frenette et al. (2007). See appendix for more details. As in our simulations of the transfer system, we do not consider potential behavioral reactions to tax changes in this way. However, given the evidence in Jessen (2016), and given that the mechanical effects on the income distribution are already very small, it is unlikely that incorporating behavioral reactions would change any of our results.

2.4.8 Specific effects of the financial crisis 2007/2008

In this section, we provide a brief discussion of the potential effects of the financial crisis 2007/2008 on the German income distribution. The financial crisis began in the year 2007 as the consequence of the bursting of the U.S. housing bubble and culminated in the breakdown of Lehmann Brothers in 2008. In Germany, the financial crisis led to a drop in GDP by 5.6 percent in the year 2009, which was followed by a swift recovery and which had surprisingly limited consequences for the German labor market (Möller, 2010). The financial crisis quickly spread across global financial markets leading to a drop in asset values and falling returns to financial assets in many countries of the world.

In relation to the years of the financial crisis, we make the following observations. These observations are not necessarily causal but they are suggestive of the potential effects of the crisis on different components of the German income distribution. First, we observe average equivalized household capital income to start falling after 2007, after continuous increases in the years before (figure 2.11b). Second, inequality in equivalized household capital incomes fell after 2007 in a marked way and did not pick up again until 2010 (figure 2.11a). Third, our index of income richness exhibited a marked fall from 2008 to 2009, also reflected in the ratio of the 90 percent percentile and the median which, however, was already falling from 2006 onwards (figure 2.1b). Fourth, the share of capital income in total household gross income slightly decreased after 2007 after continuous increases up to that point (figure 2.12). By contrast, the time series of average household labor income as well as that of inequality in household labour incomes did not exhibit obvious co-movements with the financial crisis (figures 2.7b and 2.14). This is consistent with the hypothesis that the financial crisis and the subsequent great recession had only very limited consequences for the German labor market.

— Figure 2.14 around here —

Taken together, there is little evidence that the distribution of net equivalized incomes in Germany was substantially affected by the financial crisis and the subsequent great recession. While we observe a number of more or less obvious co-movements of capital income related measures with the years of the crisis, we do not observe such co-movements for labor related measures. As the latter dominate capital incomes in their effect on the distribution of net equivalized incomes, we conclude that the net effect of the financial crisis on the distribution of equivalized net incomes was very limited.

2.5 Conclusion

This paper has considered the evolution of the distribution of equivalized net incomes in Germany after 2005. After steep increases before 2005, inequality in equivalized net incomes stayed constant or decreased over the period 2005 to 2011. We address the question of what factors were responsible for the break in the inequality trend after 2005. Our analysis suggests that the influence of the two main factors that contributed to the inequality increase before 2005, namely increases in labor income inequality and changes in the level and structure of household employment outcomes, changed substantially after 2005. First, in line with evidence from other data sources, inequality in monthly wage incomes continued to rise after 2005, although we observe a slight flattening of this trend after 2005. Second, despite the continuing trend of rising inequality in monthly wage incomes after 2005, inequality in *annual* labor incomes stopped to rise after 2005. This was the result of additional within-year employment opportunities which had an equalizing effect on the distribution of yearly net incomes. However, we show that third, apart from an equalization of within-year employment opportunities, the general expansion of employment after 2005 was such that not only households at the bottom but also households at the middle and the top of the distribution benefited. This explains why the employment boom after 2005 did not lead to a more marked fall in inequality. The third factor that was identified in Biewen and Juhasz (2012) and IAW (2013) as a contributor to the rise in inequality up to 2005 was series of tax reforms that benefited top incomes more than middle and low incomes Corneo et al. (2003). We present evidence that the changes to the tax system implemented after 2005 (a small change in the marginal tax rate for very high incomes and the introduction of a withholding tax on capital incomes) were much less substantial and did not have measurable effects on the distribution of net incomes. Finally, we show that a number of other factors that are often suspected to have influenced the distribution such as capital incomes, changing household structures, population aging, and changes in the transfer system did not significantly change the distribution after 2005.

Tables and Figures Chapter 2

Tables

Table 2.1 – Statistical significance of changes 2005/2006 to 2010/11

Index	Change	Confidence interval	
Mean (Euro/year)	685.07*	350.62	1014.49
Median (Euro/year)	744.07*	456.89	1023.40
P90/P10	-0.0480	-0.1555	0.0707
P90/P50	-0.0076	-0.0480	0.0338
P50/P10	-0.0179	-0.0641	0.0328
Theil	-0.0018	-0.0434	0.0005
MLD	-0.0073	-0.0169	0.0015
Gini	-0.0055	-0.0137	0.0026
Poverty rate	0.0004	-0.0090	0.0092
Income richness	-0.0019	-0.0078	0.0043

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.2 – Distributional effects of changing household structures

Index	Change	Confidence interval	
Mean (Euro/year)	244.88*	83.51	328.01
Median (Euro/year)	130.82*	52.09	223.58
P90/P10	0.0145	0.0000	0.0688
P90/P50	-0.0025	-0.0051	0.0211
P50/P10	0.0103*	0.0021	0.0205
Theil	0.0009	-0.0006	0.0061
MLD	0.0043	-0.0015	0.0054
Gini	0.0046	-0.0018	0.0056
Poverty rate	0.0020*	0.0002	0.0035
Income richness	0.0012	-0.0001	0.0021

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.3 – Trends in household characteristics

HH Characteristic	Mean 2005/2006	Mean 2010/2011	Confidence interval for difference	
Number of adults in HH	1.99	2.00	-0.0264	0.0252
Number of children in HH	0.69*	0.61*	-0.1114	-0.0462
Proportion women HH adults	0.53*	0.53*	-0.0136	-0.0009
Proportion foreign nationality in HH	0.09	0.09	-0.0081	0.0081
Proportion HH members with disabilities	0.11	0.11	-0.0066	0.0054
Proportion married HH adults	0.58*	0.56*	-0.0342	-0.0119
Proportion university HH adults	0.16*	0.19*	0.0231	0.0391
Proportion *Abitur/Lehre* HH adults	0.60*	0.59*	-0.0223	-0.0029
Proportion low education HH adults	0.24*	0.22*	-0.0278	-0.0093
Proportion 0-3 y. HH children	0.07*	0.05*	-0.0228	-0.0055
Proportion 4-11 y. HH children	0.17*	0.15*	-0.0261	-0.0030
Proportion 12-17 y. HH children	0.16*	0.14*	-0.0347	-0.0078
Proportion 18-30 y. HH adults	0.17	0.16	-0.0149	0.0015
Proportion 31-50 y. HH adults	0.44*	0.41*	-0.0435	-0.0174
Proportion 51-64 y. HH adults	0.19*	0.21*	0.0166	0.0346
Proportion 65 y. or older HH adults	0.21*	0.22*	0.0031	0.0200
HH in East Germany	0.21	0.21	-0.0095	0.0070

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.4 – Distributional effects of changing household characteristics

Index	Change	Confidence interval	
Mean (Euro/year)	563.67*	358.02	692.46
Median (Euro/year)	341.98*	235.73	474.01
P90/P10	0.0682*	0.0435	0.1376
P90/P50	0.0153*	0.0050	0.0457
P50/P10	0.0206*	0.0123	0.0336
Theil	0.0021	-0.0075	0.0083
MLD	0.0064*	0.0004	0.0083
Gini	0.0068*	0.0002	0.0087
Poverty rate	0.0034*	0.0016	0.0052
Income richness	0.0040*	0.0017	0.0060

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.5 – Distributional effects of changing employment outcomes

Index	Change	Confidence interval	
Mean (Euro/year)	259.33*	74.60	374.29
Median (Euro/year)	341.98*	235.73	474.01
P90/P10	-0.0575	-0.0983	0.0273
P90/P50	-0.0202	-0.0281	0.0079
P50/P10	-0.0102	-0.0312	0.0170
Theil	-0.0041	-0.0075	0.0029
MLD	-0.0000	-0.0061	0.0026
Gini	0.0002	-0.0062	0.0023
Poverty rate	-0.0023	-0.0062	0.0021
Income richness	-0.0010	-0.0033	0.0008

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.6 – Average annual employment gains conditional on current household employment status, 2005 to 2011

Current employment status	Avg. employment gain	Confidence interval	
Change in number of jobs per HH and year			
0 PT, 0 FT	0.085*	0,076	0,093
≥1 PT, 0 FT	-0.014	-0.037	0.009
0 PT, 1 FT	0.123*	0.109	0.136
≥1 PT, 1 FT	-0.069	-0.087	-0.050
≥0 PT, ≥2 FT	-0.178	-0.198	-0.157
Change in number of FT jobs per HH and year			
0 PT, 0 FT	0.035*	0.030	0.041
≥1 PT, 0 FT	0.181*	0.160	0.201
0 PT, 1 FT	-0.019*	-0.029	-0.009
≥1 PT, 1 FT	0.058*	0.043	0.072
≥0 PT, ≥2 FT	-0.221*	-0.241	-0.201
Change in number of PT jobs per HH and year			
0 PT, 0 FT	0.049*	0.043	0.055
≥1 PT, 0 FT	-0.195*	-0.215	-0.175
0 PT, 1 FT	0.142*	0.131	0.153
≥1 PT, 1 FT	-0.127*	-0.141	-0.113
≥0 PT, ≥2 FT	0.043*	0.028	0.058

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.7 – Distributional effects of changing labor market returns

Index	Change	Confidence interval	
Mean (Euro/year)	527.627*	213.90	759.92
Median (Euro/year)	636.05*	394.55	899.02
P90/P10	-0.0670	-0.1256	0.0489
P90/P50	-0.0376*	-0.0598	-0.0024
P50/P10	0.0026	-0.0276	0.0499
Theil	-0.0112*	-0.0291	-0.0020
MLD	0.0002	-0.0096	0.0045
Gini	-0.0030	-0.0117	0.0015
Poverty rate	-0.0007	-0.0055	0.0078
Income richness	-0.0047*	-0.0086	-0.0014

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.8 – Distributional effects of changing capital incomes

Index	Change	Confidence interval	
Mean (Euro/year)	88.44	-44.39	121.73
Median (Euro/year)	-26.05	-60.86	24.53
P90/P10	0	-.0151159	.046234
P90/P50	0	-.0075862	.0238904
P50/P10	0	-.0040882	.0024089
Theil	-.0008036	-.0016762	.004048
MLD	.0034816	-.0021385	.004232
Gini	.0039434	-.0023985	.0048102
Poverty rate	.0001508	-.0005058	.0005005
Income richness	.0018228	-.0000464	.0029354

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.9 – Distributional effects of changes in the transfer system

Index	Change	Confidence interval	
Mean (Euro/year)	230.41*	123.12	242.01
Median (Euro/year)	157.09*	129.91	221.06
P90/P10	-0.1234*	-0.1359	-0.0719
P90/P50	-0.0227*	-0.0250	0
P50/P10	-0.0431*	-0.0518	-0.0329
Theil	-0.0061*	-0.0066	-0.0014
MLD	-0.0021*	-0.0077	-0.0013
Gini	-0.0016*	-0.0076	-0.0012
Poverty rate	-0.0067*	-0.0082	-0.0053
Income richness	-0.0010*	-0.0014	-0.0006

Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Table 2.10 – Distributional effects of changes in the tax system

Index	Change	Confidence interval	
Mean (Euro/year)	392.86*	129.18	540.95
Median (Euro/year)	183.40*	157.00	323.99
P90/P10	0.0732*	0.0534	0.1711
P90/P50	0.0281*	0.0152	0.0746
P50/P10	0.0103*	0.0072	0.0208
Theil	-0.0148	-0.0276	0.0088
MLD	0.0025	-0.0104	0.0077
Gini	0.0047	-0.0093	0.0085
Poverty rate	0.0015*	0.0008	0.0034
Income richness	0.0071*	0.0031	0.0079

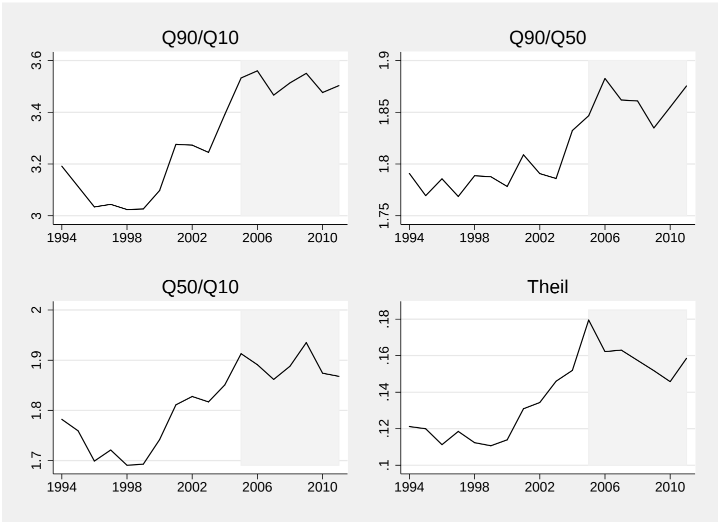
Source: SOEP 2006-2012, own calculations. *=statistically significant at 5%

95% bootstrap confidence intervals account for longitudinal design and HH-clustering

Figures

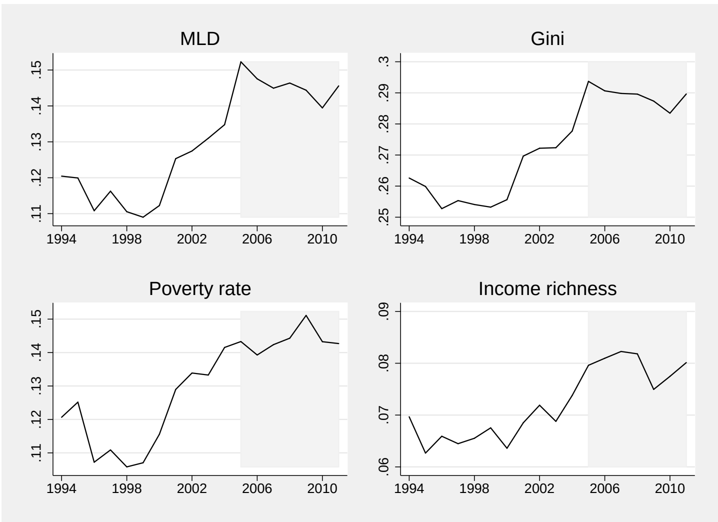
Figure 2.1 – Inequality trends 1994 to 2011

(a) Inequality trends 1994 to 2011



Source: SOEP 1995-2012, own calculations.

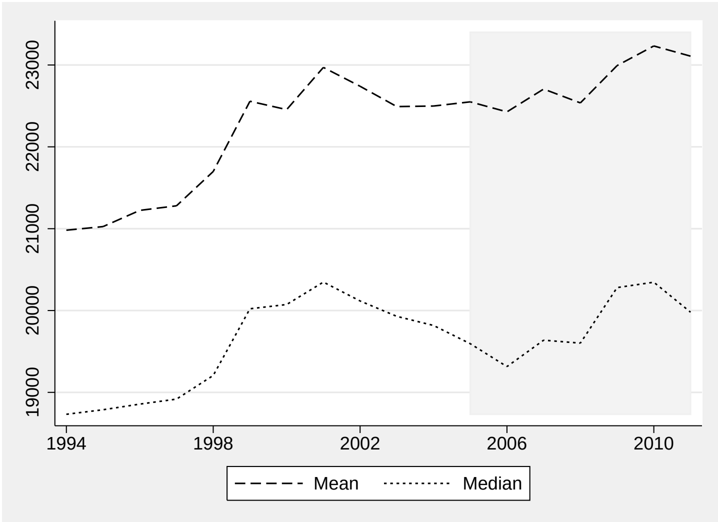
(b) Inequality trends 1994 to 2011



Source: SOEP 1995-2012, own calculations.

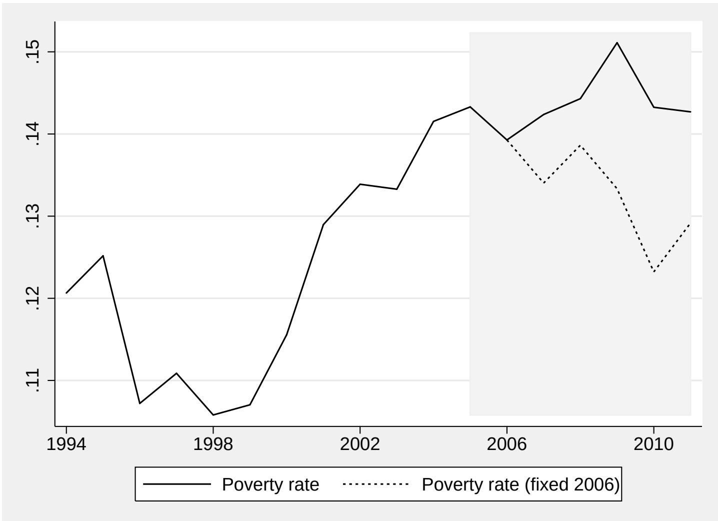
Figure 2.2 – Mean, median and poverty rate

(a) Mean and median equivalent income



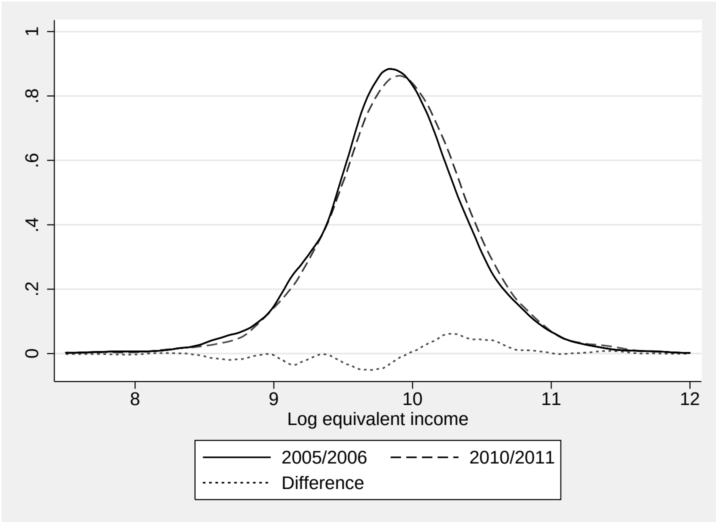
Source: SOEP 1995-2012, own calculations.

(b) Poverty rate with/without poverty line fixed at 2006



Source: SOEP 1995-2012, own calculations.

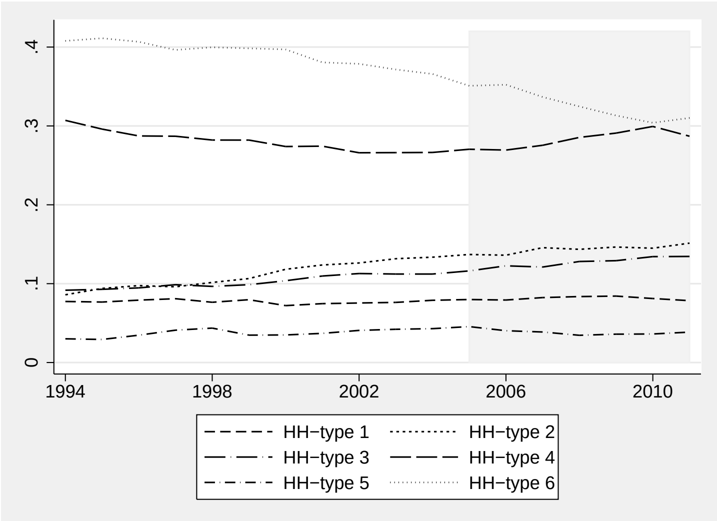
Figure 2.3 – Distribution of log equivalent incomes



Source: SOEP 1995-2012, own calculations.

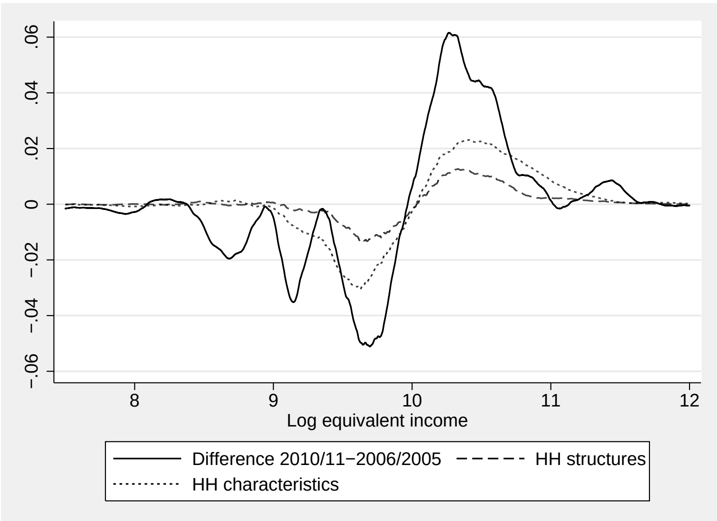
Figure 2.4 – Household structure and distributional effect of changes

(a) Changes in household structures



Source: SOEP 1995-2012, own calculations. HH-type 1=Single pensioners, HH-type 2=Multiple pensioners, HH-type 3=Single adults w/o children, HH-type 4=Multiple adults w/o children, HH-type 5=Single adults with children, HH-type 6=Multiple adults with children

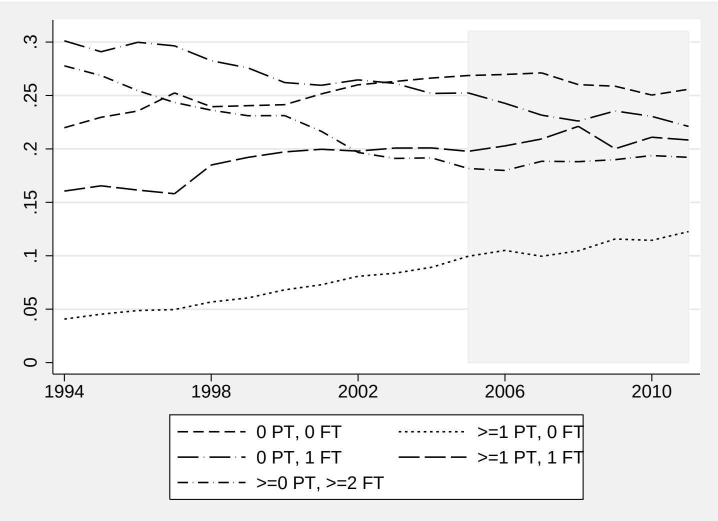
(b) Distributional effects of changing household structures
and changes in household characteristics



Source: SOEP 1995-2012, own calculations.

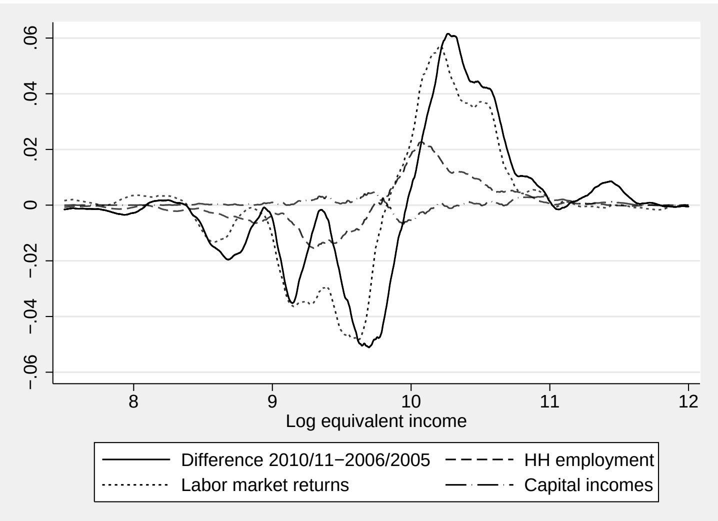
Figure 2.5 – Household employment outcomes and effect of changes

(a) Changes in household employment outcomes



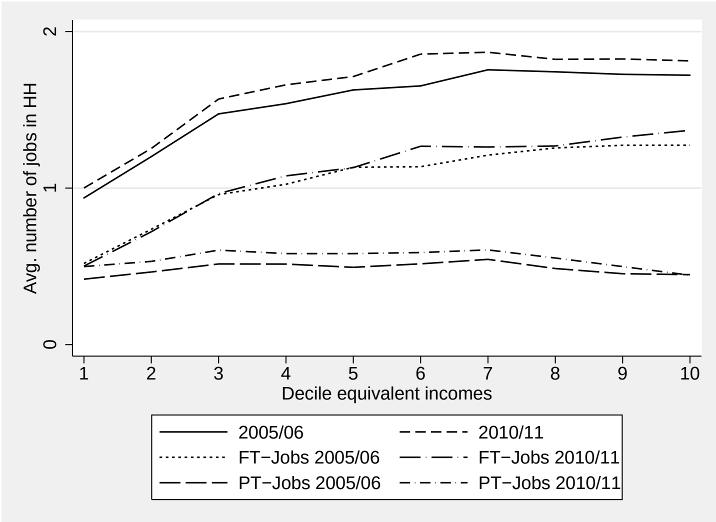
Source: SOEP 1995-2012, own calculations. FT=Full-time jobs in HH,
PT=Part-time jobs in HH (including marginal employment)

(b) Distributional effects of changing employment outcomes,
changes in labor market returns, and changes in capital incomes



Source: SOEP 1995-2012, own calculations.

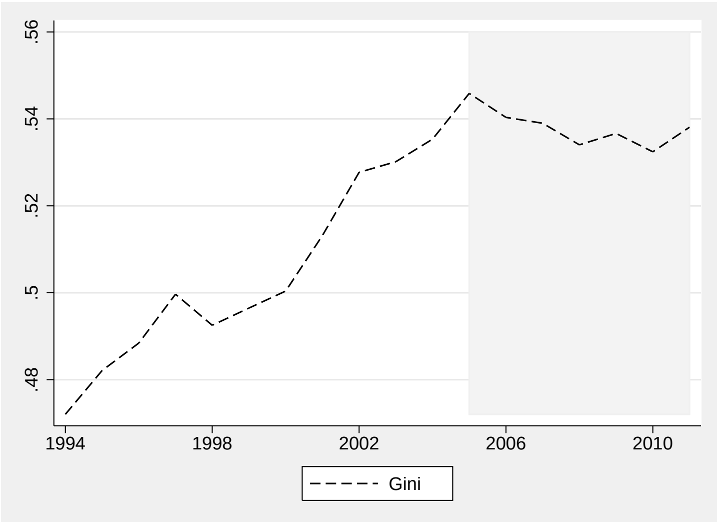
Figure 2.6 – Average number of jobs in non-pensioner households across deciles



Source: SOEP 1995-2012, own calculations.

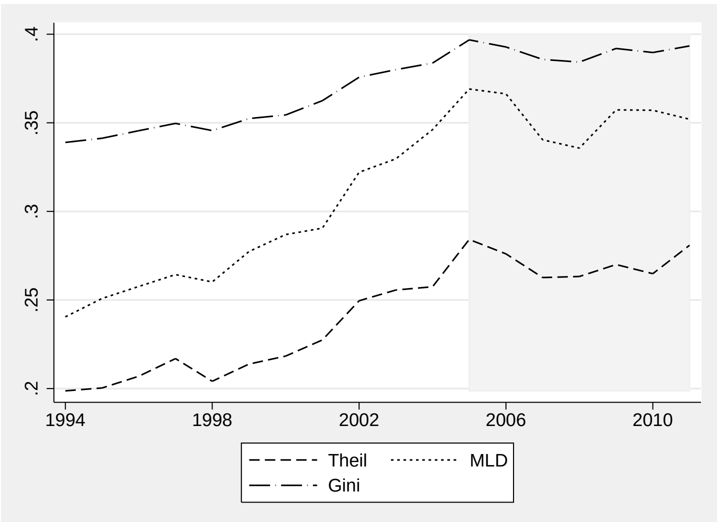
Figure 2.7 – Inequality in equivalized household labor income

(a) Inequality in equivalized household labor income (including zero incomes)



Source: SOEP 1995-2012, own calculations.

(b) Inequality in equivalized household labor income (excluding zero incomes)



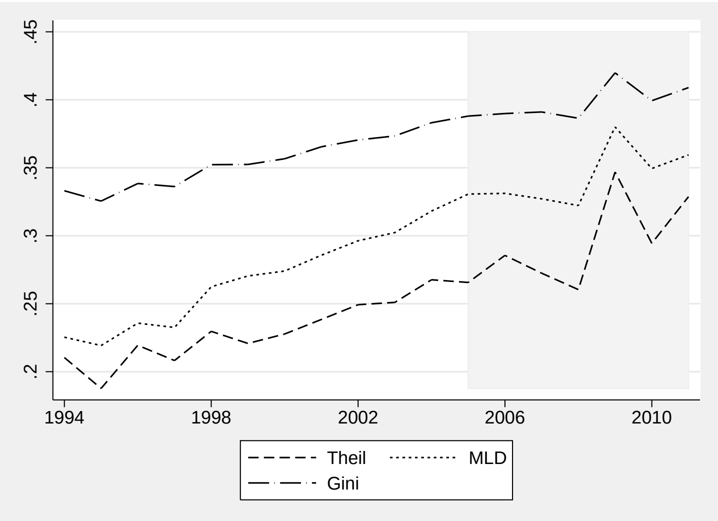
Source: SOEP 1995-2012, own calculations.

Figure 2.8 – Inequality in individual monthly labor income

(a) Inequality in individual monthly labor income (excluding zero incomes),
only full-time workers, 20-60 years old, without self-employed, civil-servants, apprentices

Source: SOEP 1995-2012, own calculations.

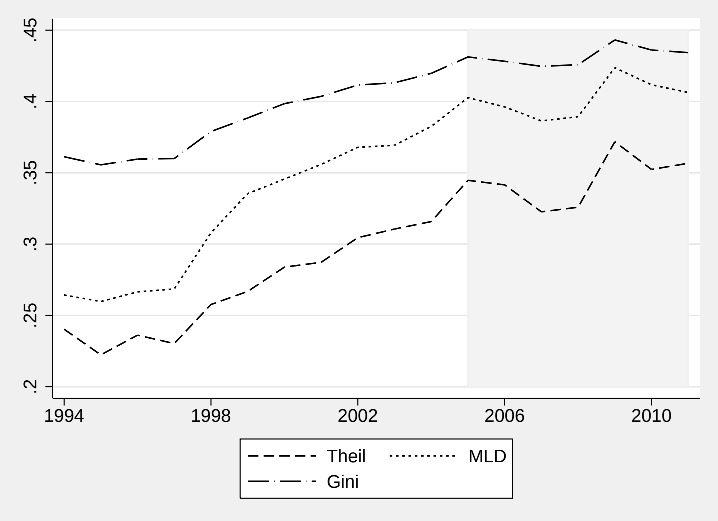
(b) Inequality in individual monthly labor income (excluding zero incomes),
20-60 years old, without self-employed, civil-servants, apprentices



Source: SOEP 1995-2012, own calculations.

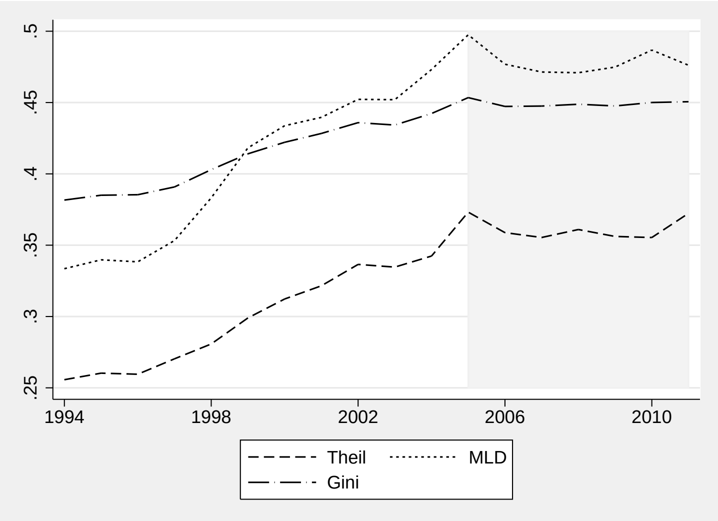
Figure 2.9 – Inequality in individual monthly labor income

(a) Inequality in individual monthly labor income (excluding zero incomes)



Source: SOEP 1995-2012, own calculations.

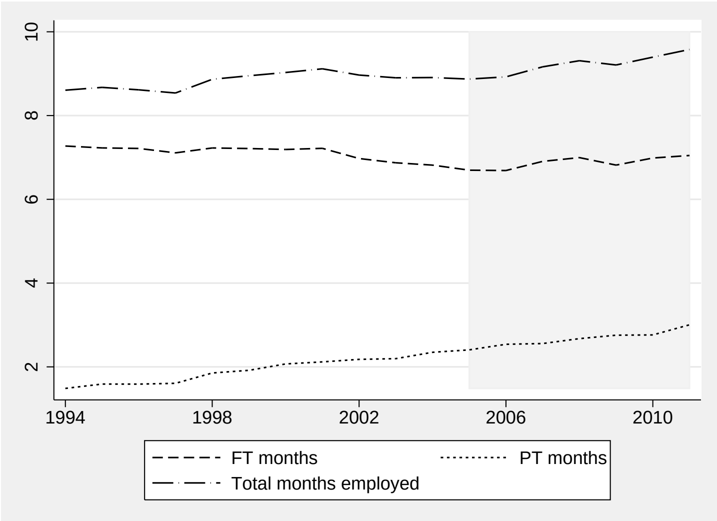
(b) Inequality in individual annual labor income (excluding zero incomes)



Source: SOEP 1995-2012, own calculations.

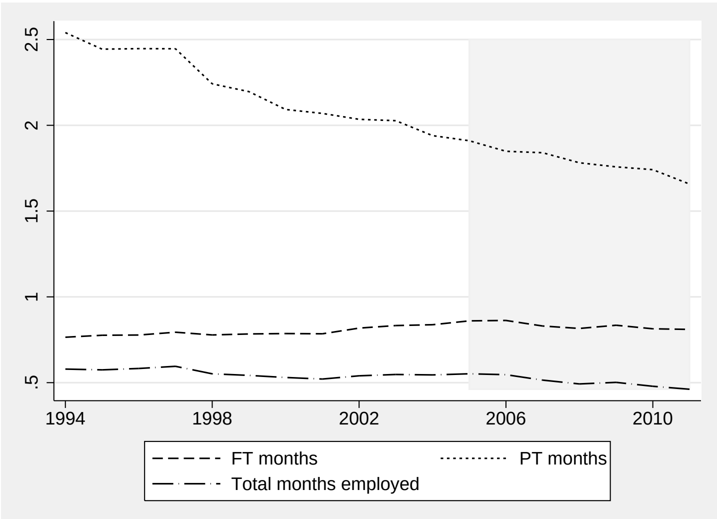
Figure 2.10 – Months worked per year and variation

(a) Months worked per year (individuals aged 20-60 years)



Source: SOEP 1995-2012, own calculations.

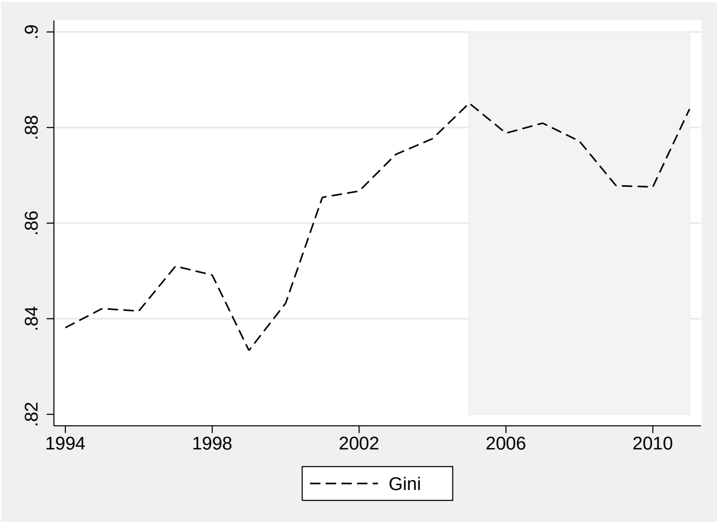
(b) Coefficient of variation of months worked per year (individuals aged 20-60 years)



Source: SOEP 1995-2012, own calculations.

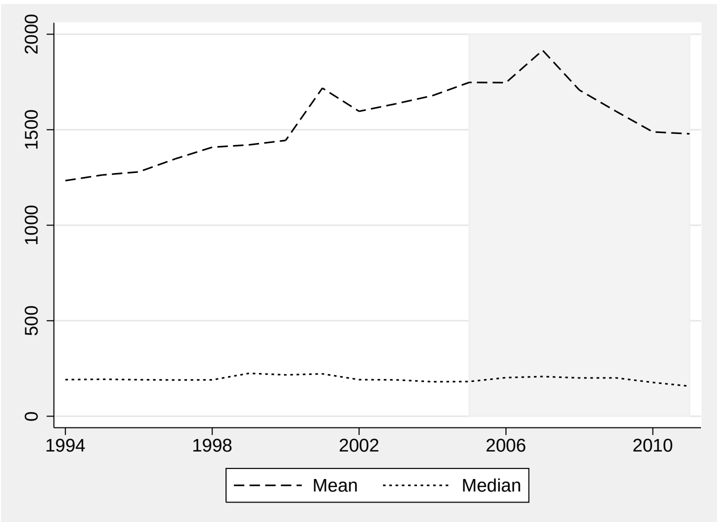
Figure 2.11 – Inequality in equivalized household capital incomes

(a) Inequality in equivalized household capital incomes (including zero incomes)



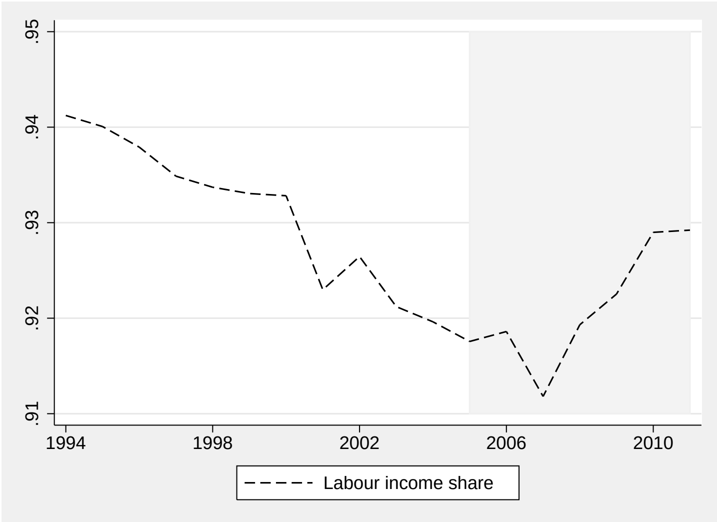
Source: SOEP 1995-2012, own calculations.

(b) Mean and median equivalized household capital income (excluding zero incomes)



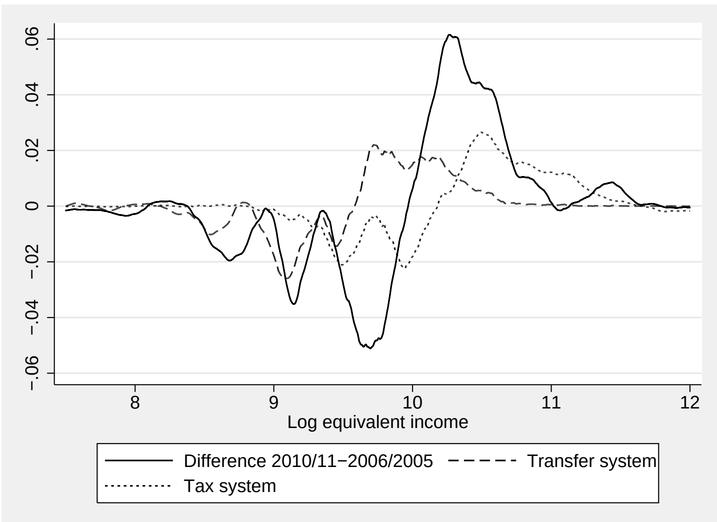
Source: SOEP 1995-2012, own calculations.

Figure 2.12 – Share of labor income in household market income



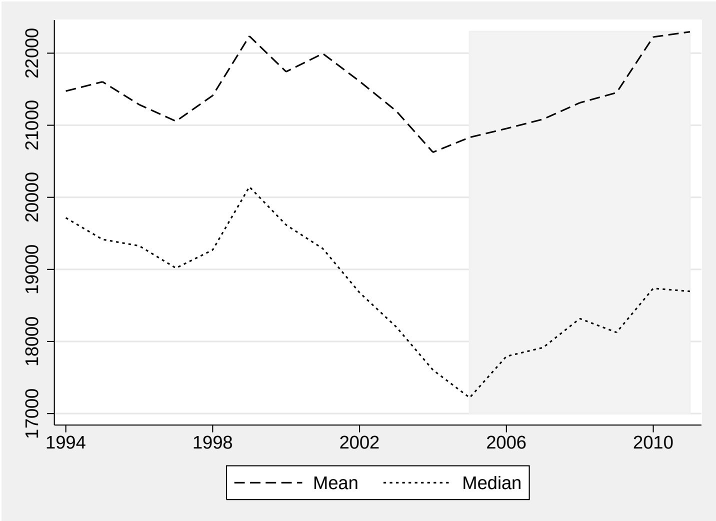
Source: SOEP 1995-2012, own calculations.

Figure 2.13 – Distributional effects of changes in transfer system and changes in tax system



Source: SOEP 1995-2012, own calculations.

Figure 2.14 – Mean and median household labor income (including zero incomes)



Source: SOEP 1995-2012, own calculations.

Appendix: Why did income inequality in Germany not increase further after 2005?

In this appendix, we document details of the different methods used to assess the influence of particular factors on the distribution of equivalized incomes. As a general note, we point out that all our computations take full account of the SOEP sampling weights.

Changes in household structure

The counterfactual distribution under the assumption that only the composition of the population with respect to household types shifts to the target period $t = 1$ (=2010/2011) but everything else is held at the level of the base period $t = 0$ (=2005/2006) is constructed as

$$f_{cf}(y) = \sum_{j=1}^{6} w_{1j} f_{0j}(y), \tag{2.1}$$

where f_{0j} is the distribution of equivalent incomes of individuals in households of type j in the base period (household types are defined in section 2.2). The counterfactual weights w_{1j} for household types j are taken to be those of the target period instead of the base period.

Changes in other household characteristics

Using the reweighting technique of DiNardo et al. (1995), the counterfactual income distribution of individuals in household type j under the assumption that only the joint distribution of household characteristics x (see table 2.3) is shifted to the target period is given by

$$f_{cf,j}(y) = \int_e \int_x f_{0j}(y|x,e)\, dF_{0j}(e|x)\, dF_{1j}(x) \tag{2.2}$$

$$= \int_e \int_x f_{0j}(y|x,e) dF_{0j}(e|x) \left[\frac{dF_{1j}(x)}{dF_{0j}(x)}\right] dF_{0j}(x), \tag{2.3}$$

where e denote the household employment outcomes defined in section 2.4.3.

The reweighting factors

$$\frac{dF_{1j}(x)}{dF_{0j}(x)} = \frac{P_j(x|t=1)}{P_j(x|t=0)} = \frac{P_j(t=1|x)}{P_j(t=0|x)} \cdot \frac{P_j(t=0)}{P_j(t=1)} \tag{2.4}$$

are computed using logit predictions $P_j(t=1|x), P_j(t=0|x)$ estimated separately for each household type j. The quantities $P_j(t=1), P_j(t=0)$ are the (weighted) sample shares of period $t=1$ and $t=0$ in the pooled sample comprising both periods $t=1$ and $t=0$. The income distribution of the overall population results from the aggregation across household types, i.e.

$$f_{cf}(y) = \sum_{j=1}^{6} w_{0j} f_{0j}(y). \tag{2.5}$$

Changes in household employment outcomes

The counterfactual income distribution of individuals in household type j that results if one keeps everything as in the base period but shifts to the level of the target period the propensity of households with characteristics x to have one of the employment outcomes e (as defined in section 2.4.3), is given by

$$f_{cf,j}(y) = \int_e \int_x f_{0j}(y|x,e)\, dF_{1j}(e|x)\, dF_{0j}(x) \tag{2.6}$$

$$= \int_e \int_x f_{0j}(y|x,e) \left[\frac{dF_{1j}(e|x)}{dF_{0j}(e|x)}\right] dF_{0j}(e|x) dF_{0j}(x). \tag{2.7}$$

The implied reweighting factors can be computed as

$$\frac{dF_{1j}(e|x)}{dF_{0j}(e|x)} = \frac{P_{1j}(e|x)}{P_{0j}(e|x)} \tag{2.8}$$

using predictions $P_{1j}(e|x), P_{0j}(e|x)$ from ordinal logit models estimated in periods $t = 1$ and $t = 0$. The ordinal logits model the probability for a household with characteristics x to have one of the employment constellations e. Again, these were estimated separately for each household type j.

Changes in labor market returns

We construct the counterfactual household net income from the perspective of the base period that takes into account that a household with characteristics x and household employment outcomes e will experience shifts $\widehat{\Delta}y_{lab}$ in the labor market remuneration of its characteristics (e, x) from period $t = 0$ to $t = 1$. The resulting counterfactual income is

$$y^{cf} = y_{gross,0} + \widehat{\Delta}y_{lab} + y_{transf,0} - y_{sscontr,0} \cdot \frac{y_{gross,0} + \widehat{\Delta}y_{lab}}{y_{gross,0}} - \ tax_0(y_{tax,0} + \widehat{\Delta}y_{lab}), \quad (2.9)$$

where $y_{gross,0}$ denotes the household gross income (i.e. labor and capital income), $y_{transf,0}$ household transfers, $y_{sscontr,0}$ household social security contributions and $y_{tax,0}$ the household taxable income of the base period. Note that the counterfactually changed household labor income also changes the social security contributions and the taxes paid by the household which is reflected in the last two terms.[21]

The shift factor $\hat{\Delta}y_{lab}$ was computed as

$$\widehat{\Delta}y_{lab} = z_0{}'\hat{\beta}_{1j} - z_0{}'\hat{\beta}_{0j}, \quad (2.10)$$

i.e. as the result of changing returns to household characteristics and household employment outcomes $z = (e, x)$ from $\hat{\beta}_{j0}$ to $\hat{\beta}_{j1}$. The labour market returns to household characteristics $\hat{\beta}_{j0}$, $\hat{\beta}_{j1}$ were computed by regressing in each period $t = 0$ and $t = 1$ household labor incomes y_{lab} on household employment outcomes e, household characteristics x, and a flexible set of their interactions. Again, the regressions were carried out for each household type separately.

Changes in capital incomes

We compute the counterfactual net household income that results under the assumption that only the location and the dispersion of capital incomes are shifted to that of the target period as

$$y^{cf} = y_{gross,0} + \widehat{\Delta}y_{cap} + y_{transf,0} - y_{sscontr,0} - \ tax_0(y_{tax,0} + \widehat{\Delta}y_{cap}). \quad (2.11)$$

[21]See section 2.5 for how we implemented the tax schedule $tax_0(\cdot)$ in our calculations.

The capital income shift factor is defined as

$$\widehat{\Delta} y_{cap} = percentile_1(rank_0)\frac{y_{cap,0}}{percentile_0(rank_0)} - y_{cap,0} \tag{2.12}$$

where $percentile_1(\cdot)$, $percentile_0(\cdot)$ are the percentiles of the distribution of household capital incomes in period $t = 1$ and $t = 0$, and $rank_0$ is the rank of the household in the distribution of capital incomes in the base period $t = 0$. Again, we take account of the changed tax burden as a result of the change in pre-tax income.

Changes in the transfer system

We compute the counterfactual household net income under the assumption that only the transfer system is shifted to the level of the target period as

$$y^{cf} = y_{gross,0} + y_{transf,0} - y_{sscontr,0} - \ tax_0(y_{tax,0}) + (IZA\Psi MOD_{transf,1} - IZA\Psi MOD_{transf,0}) \tag{2.13}$$

where $IZA\Psi MOD_{transf,1}$, $IZA\Psi MOD_{transf,0}$ are transfer payments for the given household computed by the $IZA\Psi MOD$ simulation model under the alternative transfer systems of $t = 1$ and of $t = 0$. This means each household is assigned counterfactual transfer difference $IZA\Psi MOD_{transf,1} - IZA\Psi MOD_{transf,0}$.

Changes in the tax system

The counterfactual household net income that results under the assumption that only the tax system $tax_0(\cdot)$ is changed to that of the target year $t = 1$ is defined as

$$y^{cf} = y_{gross,0} + y_{transf,0} - y_{sscontr,0} - \ tax_1(y_{tax,0}), \tag{2.14}$$

i.e. we counterfactually apply the tax system of the target period to the taxable income of the base period. For our computations, we estimated the counterfactual tax schedule $tax_1(\cdot)$ by a flexible regression of the SOEP household tax burdens (simulated by the data provider DIW) on household taxable income which results from household gross income accounting for standard exemptions and allowances. The SOEP household tax burdens incorporate all changes in exemptions and allowance as well as the introduction of the 'rich tax' in January 2007 and the introduction of the withholding tax ('Abgeltungssteuer') for capital incomes in January 2009.[22]

We emphasize that our computations regarding changes in the tax and transfer system ignore potential behavioral reactions to these changes. However, given the

[22] We thank Markus Grabka for his information on the SOEP tax simulation model.

relatively small effects and given the fact that behavioral reactions often tend to counteract the original effects (Jessen, 2016), we would not expect that taking account of behavioral reactions would change any of our results.

3 Accounting for the spouse when measuring inequality of opportunity.

3.1 Introduction

The concept of equality of opportunity (EOp) has received considerable attention since the seminal contributions of Roemer (1993, 1998), Van de gaer (1993) and Fleurbaey (1995).[1] The EOp literature is interested in the sources of inequality, distinguishing between exogenous circumstances and (partially) endogenous effort. Circumstances are defined as all factors beyond the sphere of individual control, such as parental background or gender, for which society deems that individuals should not be held responsible. Conversely, effort refers to all factors for which individuals are held responsible because they (partly) control or choose them, e.g. decisions concerning schooling or labor supply. Inequalities due to circumstances call for compensation whilst society considers inequalities due to effort to be legitimate. All previous studies of inequality of opportunity (IOp) have exclusively analyzed the impact of *individual* circumstances and choices. A factor which has not yet been studied in the literature is the relation between the personal characteristics of spouses in couples and IOp.[2] By omitting spousal information in IOp estimations, the implicit assumption in previous studies was full responsibility for the spouse's income, effort and circumstance variables. In this paper, we discuss whether or not the spouse's characteristics should be treated as responsibility factors and empirically investigate the impact on IOp estimates of explicitly incorporating the spouse's characteristics in the analysis.

Undertaking this exercise is no trivial task, at least from a philosophical point of view. The key question with relation to the EOp literature is, what should individuals be held responsible for? According to EOp theory, individuals are not responsible for their circumstances but only for their effort variables. The same is true for the spouse; he or she is not responsible for his or her circumstances but only for his or her choices. Choosing a partner, however, is a choice. Given that a spouse's circumstances are (usually) known when making the mating decision, one could argue that one should be held responsible for the circumstances of one's spouse. In fact, due to the omission of spousal variables from the analysis, this is the implicit assumption in the existing

[1] See, e.g., Ramos and Van de gaer (2016), Roemer and Trannoy (2016) or Ferreira and Peragine (2015) for recent surveys.

[2] In contrast, studies on inequality within couples look at inequality of outcomes rather than IOp. For instance, Lise and Seitz (2011) show that standard measures of inequality in terms of consumption are underestimated by about 50 percent, if one neglects intra-household inequality. For a survey, see e.g., Browning et al. (2013).

literature. However, this could be at odds with the basic EOp notion that individuals should not be held responsible for factors beyond the sphere of individual control. In other words, one could question whether it is acceptable to hold someone responsible for his or her spouse's circumstances when the spouse themself is not held accountable for their own circumstances. For effort variables, the decision becomes even more tricky. The key question here is how choices are made within couples: Do spouses make joint decisions (unitary household model) or is it rather the case that spouses bargain between their individual choices (collective household model)? In the former case, one could be held (more) responsible for the choices of one's spouse than in the latter case – especially when decisions change after marriage. We discuss these philosophical questions in further detail below and present four alternative scenarios demonstrating how a spouse's characteristics might be handled. Choosing among these scenarios entails a distinct normative choice. We empirically analyze whether, and to what extent, this choice matters for IOp estimates.

As mentioned above, by looking only at one's own circumstances, the implicit assumption in the literature produced thus far has been full responsibility for spousal variables independent of whether they constitute circumstance or effort factors. In the empirical part of this paper, we use German micro data in order to investigate the extent to which relaxing this assumption will result in changes in IOp estimates. Our results show that including spousal variables increases IOp measures by more than 20 (35) percent for gross (net) earnings. The less responsibility assumed for the partner's variables, the higher the IOp estimate. Our findings therefore suggest that deciding whether and how to account for spousal outcomes in couples is of considerable importance for IOp estimates. Assuming full responsibility, the current practice in the literature might thus result in underestimation of IOp.[3] We show that at least part of this effect originates from assortative mating, suggesting that sorting into couples cannot be ignored when measuring IOp.

This paper is organized as follows: In section 3.2, we introduce the conceptual framework for measuring IOp with couples. Section 3.3 describes the data. Section 3.4 presents the results of our empirical analysis. Section 3.5 concludes.

[3] This is even more problematic when estimating IOp for societies where marriages are arranged (e.g., by parents) and where partners themselves have a limited say in who they marry.

3.2 Conceptual Framework and Methodology

3.2.1 Measuring IOp: a simple model

In order to compare our results to previous IOp estimates, we follow standard practice to define our theoretical and empirical approaches.[4] In accordance with Roemer (1998), we distinguish between two generic determinants of outcome y_i of individual i. First, circumstances C_i are characteristics which are beyond the scope of individual control (consider race, gender, family background) and which are therefore a source of illegitimate inequalities in outcomes. Second, effort E_i represents all factors affecting earnings which are assumed to be the result of personal responsibility.[5] We focus on annual labor earnings w_i which is generated by some function f of C_i and E_i, which itself might depend on C_i: $E_i = E_i(C_i)$.

$$w_i = f(C_i, E_i(C_i)). \tag{3.1}$$

Following Ferreira and Gignoux (2011) and Niehues and Peichl (2014), we employ the ex-ante approach of EOp[6] by partitioning the population of agents $i \in \{1, ...N\}$ into a set of disjunct types $\Pi = \{T_1, T_2, ...T_k\}$, i.e. subgroups of the population which are homogeneous in terms of their circumstances. The income distribution within a given type is a representation of the opportunity set which can be achieved for individuals with the same circumstances C_i by exerting different degrees of effort. Following the utilitarian reward principle Fleurbaey and Peragine (2013); Ramos and Van de gaer (2016), perfect EOp is achieved if the mean advantage levels μ are identical across these types, i.e., $\mu^k(w) = \mu^l(w), \forall l, k | T_k, T_l \in \Pi$. In our case, this corresponds to identical mean wages across types. Measuring IOp thus means capturing the extent to which $\mu^k(w) \neq \mu^l(w)$, for $k \neq l$. To compute a measure of IOp, Checchi and Peragine (2010) suggest constructing a hypothetical smoothed distribution Foster and Shneyerov (2000), $\mu^k(w)$, which is obtained when each individual outcome, w_i^k, is replaced by the group-specific mean for each type, $\mu^k(w)$.

[4] The notation closely follows Niehues and Peichl (2014).

[5] As is common in the majority of EOp literature, we do not explicitly take into account the role of luck in our estimations. Hence, we (implicitly) assume that luck belongs to the sphere of individual responsibility and in our deterministic model, the individual is held responsible for any random component that may affect his income and that cannot be attributed to the observed circumstances. The same is true for potential measurement errors in the earnings data. See Lefranc et al. (2009) for the extension of the EOp framework in order that it explicitly take luck into account.

[6] In the (empirical) EOp literature, two different approaches have been used to estimate IOp Fleurbaey and Peragine (2013): ex-ante vs. ex-post. The former partitions the population into types, i.e. groups of individuals endowed with the same set of circumstances, and IOp is measured as inequality between types. In the latter case, individuals are classified into responsibility groups (tranches) of individuals at the same effort level and inequality within tranches is investigated.

Based on this smoothed distribution, we calculate for any (scale invariant) inequality index I the absolute IOp level (IOL) $\theta_a = I(\{\mu_i^k\})$. While the idea of opportunity egalitarianism is to look at IOp and not at total inequality in outcomes, it might still be informative to look at the relative share of total inequality that can be attributed to circumstances, i.e. the IOp ratio (IOR) $\theta_r = \frac{I(\{\mu_i^k\})}{I(w)}$. This approach allows the total income inequality to be decomposed into inequality within types (i.e. effort inequality) and inequality between types (i.e. opportunity inequality). Due to data limitations, we restrict our analysis to between type inequality since cell sizes of types tend to become smaller with increasing numbers of regressors. In order to respect the axioms of anonymity, Pigou-Dalton transfer principle, normalization, population replication, scale invariance and subgroup decomposability, we choose a member of the Generalized Entropy class Shorrocks (1980) as our inequality measure (see, e.g., Ferreira and Gignoux (2011) and Ramos and Van de gaer (2016) for a discussion of measures and axioms). By introducing the further requirement of path-independent decomposability (see Foster and Shneyerov (2000) for a thorough discussion of this and the other axioms the GE measures satisfy), the set of eligible indices reduces to the mean log deviation (MLD) $I_0 = \frac{1}{N}\sum_i \ln \frac{\mu_w}{w_i}$.

3.2.2 Responsibility for the mating choice?

The key question in the literature on EOp is what individuals should be held responsible for, i.e. where to draw the responsibility cut-off (see, e.g., the discussion in Ramos and Van de gaer (2016), Roemer and Trannoy (2016) or Ferreira and Peragine (2015)). At the extremes of the range of potential cut-offs, hard determinists, on one end, would deny the existence of free will and the resulting set of responsibility factors would be empty. On the other end, following the self-ownership argument by Nozick (1974), individuals are entitled to the products of all personal characteristics (including genetic characteristics such as innate talent) and the set of circumstance factors would be empty, i.e. all inequalities based on legal activities are legitimate. Between these two extremes, two main concepts of responsibility emerge in the EOp literature Ramos and Van de gaer (2016). On one hand, individuals could be held responsible only for what lies within their control Arneson (1989); Cohen (1989); Roemer (1993, 1998). On the other hand, individuals could be held responsible for their preferences and the choices that follow from those preferences Dworkin (1981a,b); Parijs (1995); Fleurbaey and Maniquet (2008).

As mentioned above, we follow Roemer (1998) and assume that individuals should not be held responsible for circumstances beyond the sphere of individual control. An open question following from this assumption in the EOp literature is, however, whether an individual in a relationship is responsible for his or her partner. So far, the literature does not explicitly consider the impact of partners, i.e. in empirical applications only *own* circumstances are included. Therefore, the current baseline case is full responsibility for the spouse's effort and circumstance variables (henceforth labeled as *(i) Full responsibility*) as these variables are implicitly treated as (unobserved) effort. We use this case as a benchmark in order to compare various possible scenarios which differ in terms of responsibility for the spouse's income, effort and circumstance variables. This baseline case can be rationalized as follows. Individuals are not responsible for their own circumstances but only for their own effort variables. The same is true for the spouse. He or she is not responsible for his or her circumstances but only for his or her choices. Choosing a partner, however, is a choice. A spouse's circumstances are (usually) known when committing to a relationship and are therefore an individual choice in the sense that individuals are fully aware of the pre-determined circumstances of their partner. One can also argue that an individual is responsible not only for the spouse's circumstances, but also for the spouse's choices. This is the case in an unitary household model where the household acts as one unit and both partners jointly decide on, e.g., labor supply, effort, and hence income Chiappori and Meghir (2015).

It is true, at least in developed Western societies, that the "choice" of a partner is indeed considered to be a true individual *choice*. However, from a historical perspective this is a relatively recent development. Furthermore, while the mating itself can be argued to be a choice, the interpretation for the effort variables of the partner is less clear as these are subject to potential changes after marriage. Hence, they are arguably outside the sphere of individual control of the spouse. In addition, the circumstances and effort of the partner may affect the individual's own effort and may therefore have an indirect effect on own earnings. On the one hand, well educated or hard working spouses may motivate their partner to engage in similar activities, while low effort partners may discourage their spouses from exerting higher degrees of effort. On the other hand, individuals with hard working partners may feed from their spouse's effort by adjusting their labor supply. In the following paragraphs, we present three relaxations of the baseline case assumption of full responsibility. In this respect, we analyze to what extent accounting for partners' circumstance and effort variables matters when measuring IOp.

The first deviation from the baseline case can be rationalized as follows. It is well documented that wage setting institutions and potential wage discrimination are important determinants of individual earnings. While these factors might (or might not) be known to the individual, there is little he or she can do to influence them. Moreover, spouses' earnings are correlated due to assortative mating Schwartz and Mare (2005); Pestel (2016) and, at least in Germany (but also France and the US), directly related due to joint taxation. The higher income of the first earner directly incentivizes the second earner to adjust his/her labor supply and income and vice versa. Hence, one can argue that individuals should not be held responsible for the spouse's wage earnings. Given, however, that circumstances are known, one could be held responsible for these factors. In fact, within a unitarian household framework, consumption of both spouses enters the joint household utility function in the same uniform way with identical welfare weights. The household is therefore assumed to maximize a common utility, leaving no room for bargaining or negotiations between household members. In such a framework, it can be argued that the members of an household acting as one unit are responsible for the joint actions and effort. We therefore define the second scenario as responsibility for the spouse's circumstances and choices, but not the spouse's wage (*(ii) Responsible for partner's circumstances and effort (unitary model)*).

While the unitary model of joint decision on labor supply and effort within a household is one possibility, the second scenario relies on the collective household model. Here, however, the consumption of each household member enters the utility function with potentially differing welfare weights, leaving room for differing preferences and intra-household bargaining. In this case, each partners individually exerts individual effort in terms of their labor market activity before bargaining about the distribution of joint household income. In such a framework, the effort variables of the partner are not the choice of the individual; rather, they are determined solely by the spouse, possibly after committing to a relationship. The third scenario is therefore responsibility for the partner's circumstances, but not for his or her income and choices (effort) (*(iii) Responsible only for partner's circumstances (collective model)*).

Finally, one can argue that individuals should not be held responsible for the circumstances, choices or income of their partner. There are two arguments which might support this scenario. First, the notion of IOp is that nobody should be held responsible for things beyond the sphere of individual control. As we cannot control the circumstances of our partner, except by leaving him or her or by refusing to mate, a partner's circumstances can thus not be considered as an own choice.[7] Second, the choice set of available partners is not necessarily identical for all individuals. One observation of the existing literature on couples and family economics is the phenomenon of increased assortative mating. Hence, higher educated individuals increasingly mate with other highly educated individuals Schwartz and Mare (2005). While high-skilled individuals may choose from a pool of both low- and high-skilled partners, potential partners of low-skilled individuals are mostly limited to other low skilled individuals. This leaves

[7]In addition, one might not be fully aware of a spouse's full set of circumstances due to asymmetric information when committing to a relationship.

high and low skilled individuals with different choice sets. Combining both arguments, our fourth scenario is no responsibility whatsoever for the spouse's characteristics at all (*(iv) No responsibility*).

To sum up, the choice between these different levels of responsibility for one's partner cannot be answered without moral judgment and consideration of the context of the research question. In this paper we analyze whether these different scenarios differ empirically.

3.2.3 Empirical strategy to estimate IOp

Baseline case

In our empirical estimation approach, we use the same data, sample selection and parametric specification as Niehues and Peichl (2014) to estimate IOp.[8] Relying on a parametric approach allows us to estimate the impact of numerous circumstance variables even in the presence of small sample and cell sizes.[9] Log-linearization of equation (3.1) and adding an error term yields the following empirical specifications[10]:

$$\ln w_i = \alpha C_i + \beta E_i + u_i, \tag{3.2}$$

$$E_i = \kappa C_i + v_i. \tag{3.3}$$

Equation (3.2) represents the direct effect of circumstances on income while equation (3.3) models the indirect effect of circumstances on income through effort. Since it is unlikely that we will observe all relevant circumstance and effort variables that shape individuals' outcomes, estimating this model will likely yield biased estimates. However, in order to compute IOp shares, it is not necessary to estimate the structural model and to derive causal relationships. By substituting the effort equation (3.3) into

[8]In empirical estimations of EOp, it is impossible to observe all characteristics that constitute an individual's circumstances (e.g. innate talent or ability). Hence, existing estimates of IOp are only lower bound estimates of the true share of unfair inequalities due to circumstances Ferreira and Gignoux (2011). Exceptions are Bourguignon et al. (2007) who simulate the magnitude of omitted variable bias to estimate bounds around the true effect of observed circumstances on income inequality and Niehues and Peichl (2014) who suggest an upper bound estimator.

[9]In contrast, non-parametric methods avoid the arbitrary choice of a functional form on the relationship between outcome, circumstances and effort (e.g. Lefranc et al. (2008), Ferreira and Gignoux (2011) or Aaberge et al. (2011)). However, this approach has the drawback that considering more than one circumstance variable is difficult due to practical reasons in the presence of small cell sizes which is usually the case in survey data. Access to large-scale administrative panel data with information on circumstances (family background), which is not available in Germany, would allow to estimate IOp also non-parametrically.

[10]We use the log of incomes since estimation in logs is common in labor economics as log-normal is typically a very good fit for (right-skewed) earnings data. Nevertheless, we have also estimated the models in levels (as robustness checks; not reported) and did not find systematic differences.

the earnings equation (3.2), we obtain the following reduced-form relationship:

$$\ln w_i = \underbrace{(\alpha + \beta\kappa)}_{\psi} C_i + \underbrace{\beta v_i + u_i}_{\eta_i}. \tag{3.4}$$

This equation can be estimated by OLS to derive the fraction of variance which is explained by circumstances. If all observed circumstances, C^K, are included in the equation (3.4), then the estimate $\widehat{\psi}$ measures the overall effect of circumstances on labor earnings, combining both the direct and indirect effects. On this basis, we can construct a parametric estimate of the smoothed distribution:

$$\widetilde{\mu}^{LB} = \exp[\widehat{\psi} C_i^K + \sigma^2/2]. \tag{3.5}$$

As we replace earnings by predicted earnings (with σ^2 being the estimated residual variance in the earnings equation, see Blackburn (2007)), all individuals with the same circumstances necessarily have the same advantage levels. Thus, in the case of absolute EOp, i.e. no income differences due to (observed) circumstances C_i^K, all predicted earning levels would be identical. IOp can subsequently be measured as the inequality of these counterfactual earnings levels, where remaining income differences are due only to differences in circumstances.

Spouses and IOp

We analyze the impact of partners' personal characteristics on IOp by extending the baseline measure in three steps. The current approach in the literature (according to equation (3.4)) implicitly assumes full responsibility for the spouse's variables as these are not included as circumstances in the regression and are hence treated as unobserved effort. This baseline case serves as a benchmark for further specifications *(ii) - (iv)*. The second case assumes responsibility for the spouse's circumstances and effort variables (*(ii) Responsible for partners' circumstances and effort (unitary model)*). This is empirically implemented by adding the earnings of the partner, $\ln w_i^P$, to equation (3.4), treating them as circumstances of the individual:

$$\ln w_i = \psi C_i + \zeta \ln w_i^P + \eta_i. \tag{3.6}$$

The definition of the third scenario (*(iii) Responsible only for spouse's circumstances (collective model)*) proceeds analogously. In order to hold the individual responsible for the circumstance variables of the partner, one must add the earnings and effort

(E_i^P) variables of the partner to the wage regression (3.4):

$$\ln w_i = \psi C_i + \zeta \ln w_i^P + \lambda E_i^P + \eta_i. \tag{3.7}$$

Finally, the case of full responsibility adds all income, effort and circumstance (C_i^P) variables of the partner to the wage equation (*(iv) No responsibility*). In this case, the partner is fully accounted for in the wage regression. Hence, all of his or her personal characteristics are treated as circumstances of the individual:

$$\ln w_i = \psi C_i + \zeta \ln w_i^P + \lambda E_i^P + \phi C_i^P + \eta_i. \tag{3.8}$$

To sum up, while in equation (3.4) all partner variables are (implicitly) treated as individual effort, this changes in the following equations. In the end, in equation (3.8), all partner variables are treated as own circumstances. Hence, comparing equations (3.4) with (3.6) – (3.8) gives the full range of not accounting for spousal characteristics to fully accounting for spousal characteristics in IOp estimations.

Since in the literature on IOp measurement, individual as well as household income definitions are used, we replace individual income w_i with household (equivalized) income $y_h = (w_i + w_i^P + \omega_h)/m$ (where m is the modified OECD equivalence scale and ω_h represents non-labor household income) in an extension.[11] Thereby, we can explore to what extent our results are driven by intra household inequality.

3.3 Data

For our estimations, we use the 2013 version of the German Socio-Economic Panel (SOEP) (2013). The SOEP is a representative panel survey of households and individuals in Germany.[12] For our analysis, we use information from 1991 until 2011, i.e. the the period following the German reunification.

In line with the previous literature and especially following Niehues and Peichl (2014), we define our units of analysis as individuals aged 25-55 with data on parental background. We exclude singles from our analysis since we are interested in the relation between partners' characteristics in couples. The dependent variables are the logs of gross and net real labor earnings, adjusted by consumer price indices. In the sensitivity analysis, we extend the income definition to net equivalized household income using the modified OECD equivalence scale. Inequality measures are based on the corresponding

[11]Note that for this specification we cannot estimate equation (3.6).

[12]A detailed overview of the SOEP is provided by Haisken De-New and Frick (2005) and Wagner et al. (2007). Issues concerning sampling and weighting methods or the imputation of information in case of item or unit non-response is well documented by the SOEP Service Group.

absolute levels of earnings.

As *circumstance variables,* we include gender, the year of birth of the individual, dummies for whether the individual was born in a foreign country or in East Germany, the degree of urbanization of the place where the individual was born, categorical variables indicating the occupation and education of the individual's father as well as the height of the individual. The occupation of the father is categorized into five groups. The omitted benchmark is blue-collar. In addition, farmer, white collar, free-lancer, highly qualified white collar worker, and civil servant form separate groups. The education of the father is categorized into four groups, with no degree serving as the benchmark case. Secondary education, intermediate education, and higher education are used to define the other groups. Summary statistics on mean annual earnings and the main circumstance variables are reported in table 3.3 in the appendix. The *effort variables* used in the different specifications are work experience, working hours, education, and industry. Work experience is measured in years; working hours is measured in in weekly hours. The education of the individual is measured in years. In order to capture industry effects, eleven categories of industry are formed.[13] 3.2 gives a detailed description of the variables used.

3.4 Empirical results

3.4.1 Estimation of earnings equations

We begin our analysis by regressing the log of earnings for each year on all available circumstance variables which are expected to have an impact on labor earnings (equation 3.4). The results for the baseline scenario as well as the case of no responsibility in gross and net annual earnings are reported in tables 3.5 - 3.12 in the appendix. Using the baseline case (i), we can identify the well-documented gender wage gap in gross and net wages. Although the gender pay gap is gradually declining over time, women continue to receive significantly lower wages in comparison to male colleagues. Being an immigrant or being born in Eastern Germany has a negative impact on wages. Individuals with highly educated fathers or parents working as civil servants have higher wages compared to individuals with blue-collar or self-employed parents. Being born in larger cities, as opposed to the countryside, is also associated with higher wages. The educational degree of the father has a strong but ambiguous effect.

Controlling for the spouse's income in equation (3.6) yields the case of responsibility

[13]The eleven groups are public administration and social security, which serves as the benchmark, fishery and agriculture, energy, chemicals and steel, engineering, manufacturing, construction, whole-sale and trade, transport, financial industry, service, education, and health service.

for spouse's circumstance and effort variables (*ii*). The income of the partner has a significant negative effect on personal earnings. At first glance, this seems to contradict previous findings of increased assortative mating. However, our analysis is carried out in terms of annual earnings. Taking into account the fact that share of part-time workers is about 40 percent for females and only 5 percent for men, the observed negative correlation is less surprising. In addition, we find a positive correlation for hourly wages. Comparing gross and net figures, we find systematically higher coefficients for net earnings. This indicates a possible effect of the German system of joint income taxation.

Controlling for effort and income variables of the partner, we assume responsibility only for spouse's circumstances (*iii*), as implemented in equation (3.7). A higher level of education on the part of the partner is associated with higher earnings of the individual. The coefficient increases over time and is significant since 1994. In line with the literature on assortative mating, we find a positive correlation between spouses' education. The industry in which the partner works has no clear effect on individual earnings. Figures for gross and net earnings show similar results for this specification.

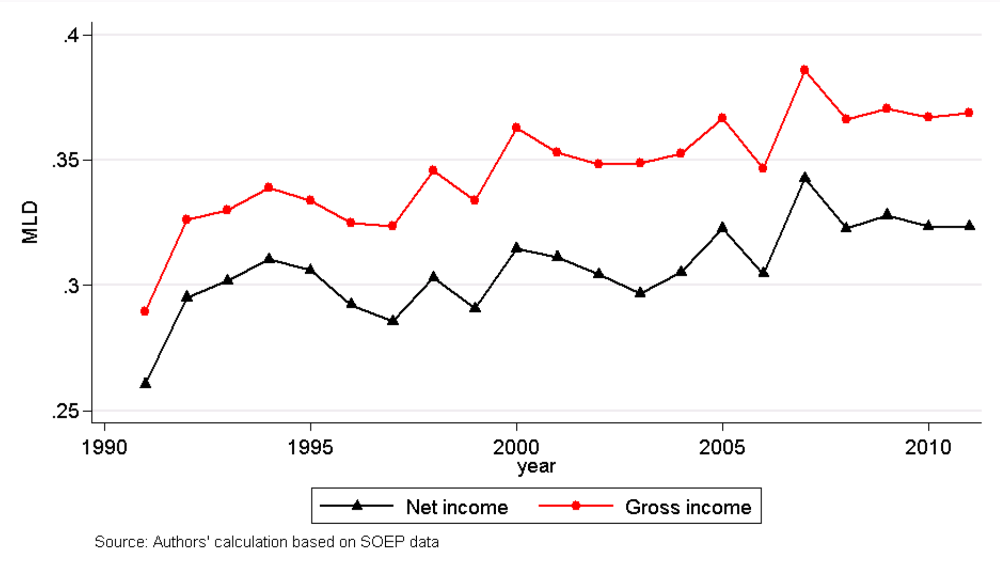
The specification of no responsibility for the partner (*iv*) is implemented by controlling for all available partner information in equation (3.8). The partner's circumstance variables display different patterns compared to the individual's own variables. A higher educational degree of the father has a negative effect from 1991 until 1999, with significant values only in 1998 and 1999. Past 1999, the degree of the father has a small and insignificant negative effect on earnings and has even turned positive since 2001. Having a foreign partner or a partner from East Germany has no significant effect on individual earnings. If the partner has a father working as a professional, e.g. as a highly skilled manager, this is associated with higher earnings, while a paternal occupation in agriculture corresponds to lower earnings.

3.4.2 Outcome inequality

Inequality in gross and net earningsas measured by mean log deviation (MLD) is depicted in figure 3.1 and shows a generally increasing trend for both gross and net earnings. Inequality increases from 0.28 to 0.36 between 1991 and 2011. The strongest increases are found between 1991/1992 and 2006/2007 with increases of 0.036 and 0.039 points, respectively. Inequality in gross earnings is significantly larger than in net earnings. Here, MLD increases from 0.26 in 1991 to 0.32 in 2011.

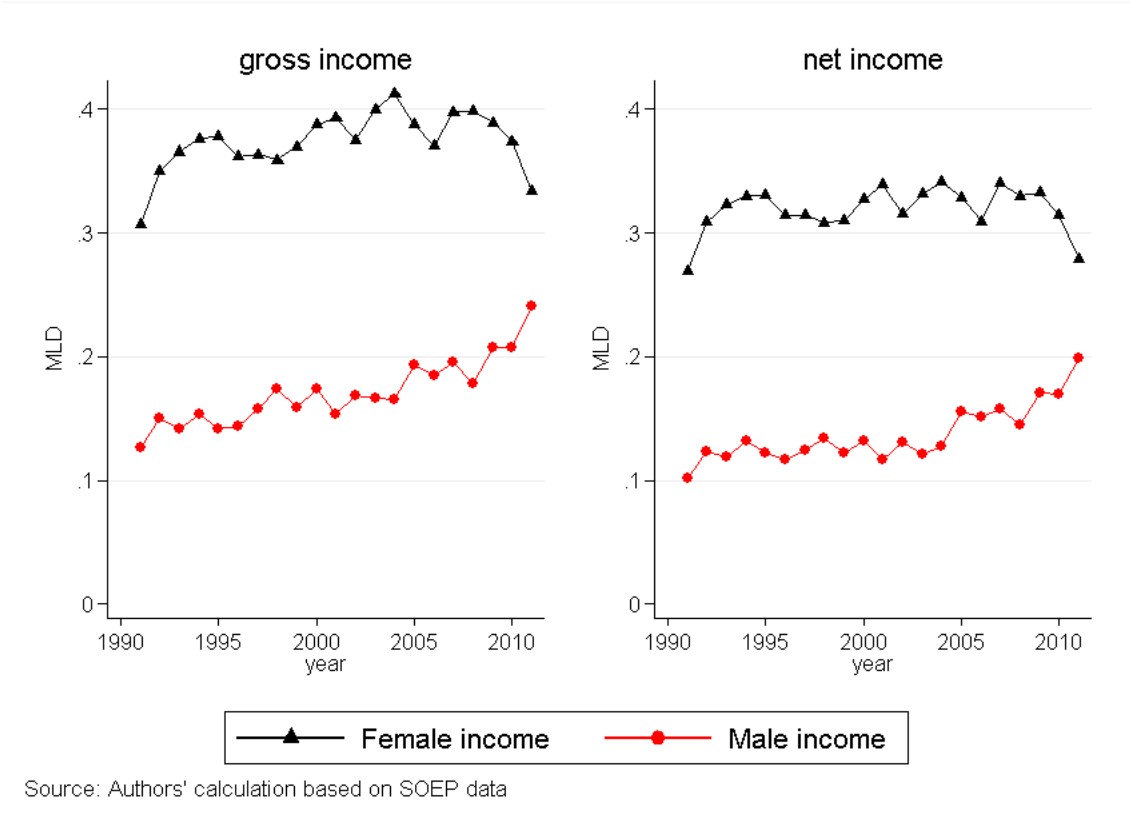
Comparing male and female gross earnings in figure 3.2, we find higher levels of inequality among female earners. However, while MLD for men is steadily increasing,

Figure 3.1 – MLD for annual income - gross vs. net



it shows an inverse U-shaped pattern for women. The MLD for men increases from 0.12 in 1991 to 0.24 in 2011. Values for the female sample are 0.30 in 1991 and 0.33 in 2011, with a peak of 0.41 in 2004. Using net instead of gross earnings for the comparison of the male and female sub-samples, we find similar trends. The values in this case are, however, generally lower, as inequality in net earnings is lower than in gross earnings.

Figure 3.2 – MLD for annual income - male vs. female

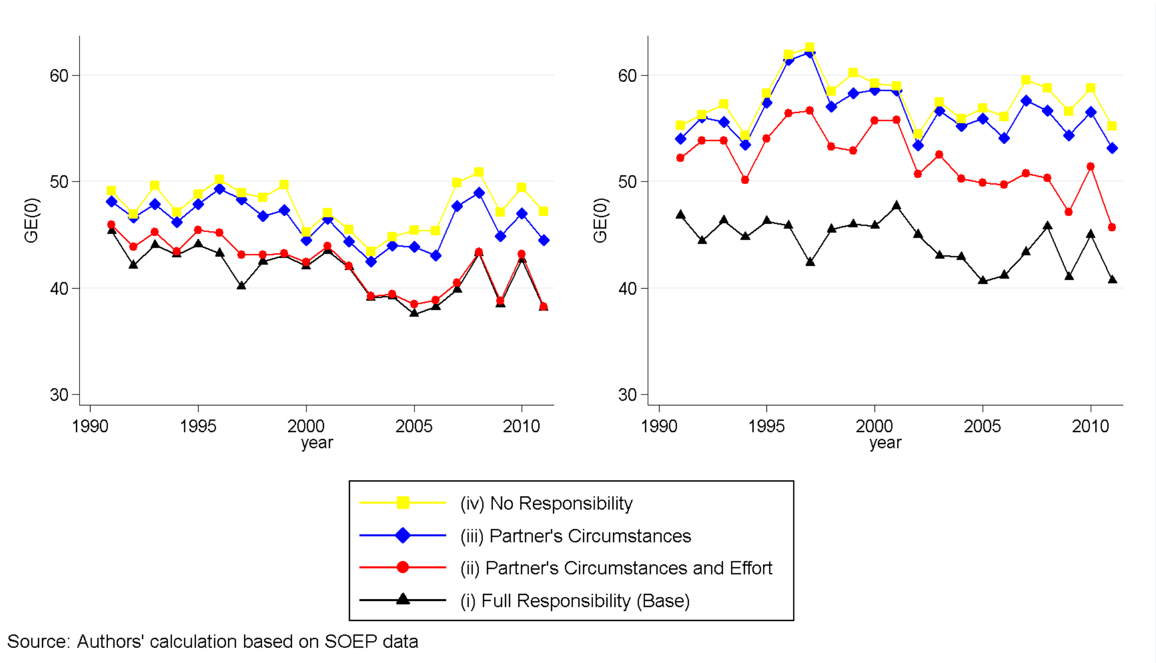


3.4.3 Inequality of opportunity ratios

The lower bound level of IOp (IOL) is estimated by computing the MLD of the predicted values from the earnings equation. We do this for specifications (3.4) and (3.6) – (3.8), using gross and net annual earnings. The results are shown in appendix 3.5. Estimating the IOp ratio (IOR) is straightforward; we simply divide the IOL by total inequality in annual earnings. Starting with the comparison of gross and net annual earnings, we further distinguish between male and female sub-samples. The results for the full sample are shown in figure 3.3. The black line displays the baseline case from equation (3.4). The red line corresponds to the model which includes partner's circumstance variables shown by equation (3.6). The case of the model from equation (3.7), including both the spouse's circumstance and effort variables, is displayed by the blue line. Finally, the full specification including circumstances, effort and income variables of the partner corresponds to the yellow graph (equation (3.8)).

Looking at gross and net earnings displayed in figure 3.3, we find that the IOR in gross earnings was decreasing from 1991 until 2005. The majority of the decrease in the IOR can be said to have been driven by the increase in overall earnings inequality rather than a decreasing IOL. While the baseline case of full responsibility sees the IOR decrease from 45.3 to 37.5 percent, the specification using all available partner infor-

Figure 3.3 – IOR for annual income - gross vs. net

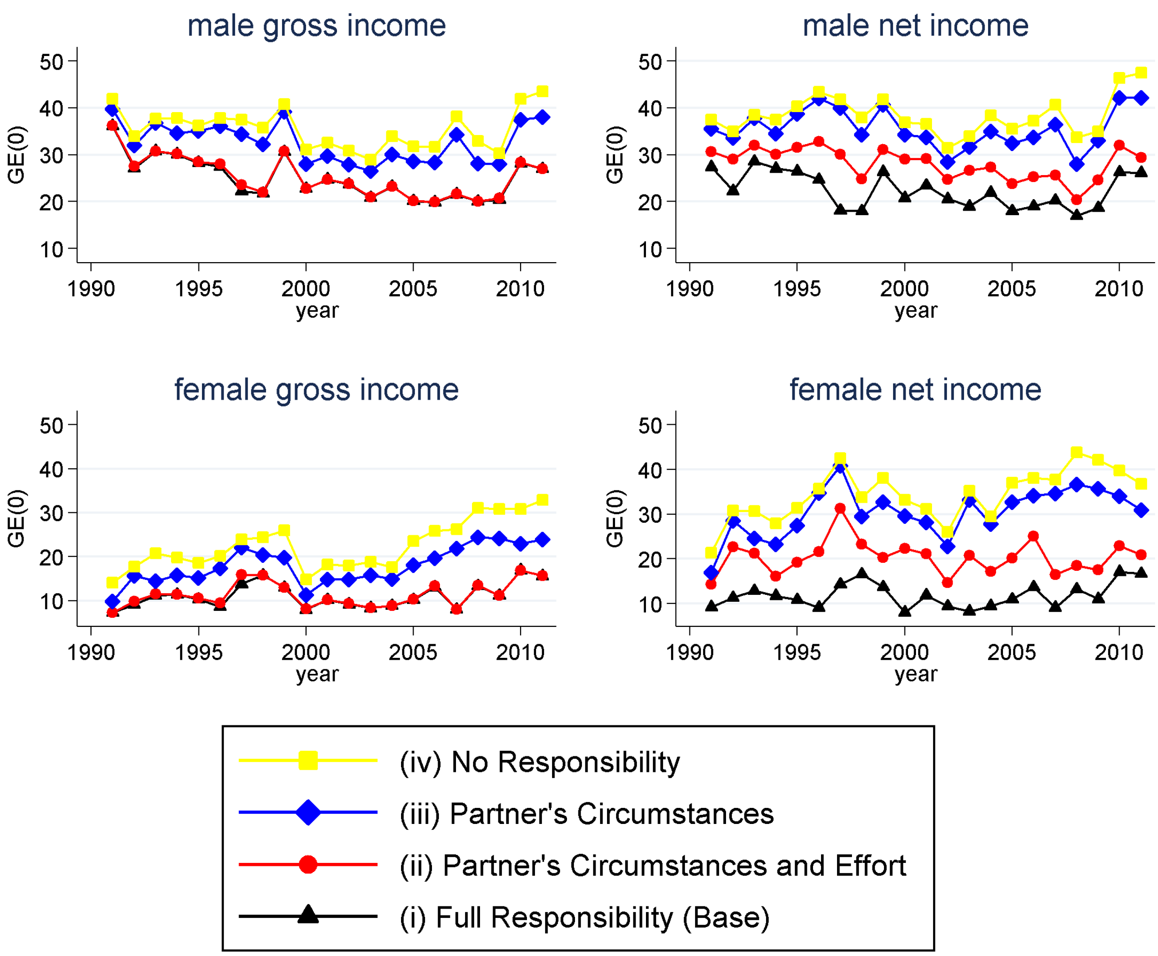


mation sees the IOR decrease from 49.1 percent to 43.3 percent in 2003. Afterwards, we see an increase in IOR to 50.8 percent in 2008 and strong fluctuations thereafter until 2011. Our results show that accounting for spouse's variables can be important when measuring IOp, as including them increases the lower bound of IOp. While IOR for the baseline case is 38.1 percent in 2011, the full specification yields a value of 47.1 percent. Hence, IOR is 22 percent larger when all information available about the partner is taken into account. The less responsibility is assumed, the higher the IOp measure.

Looking at the baseline case for net earnings, we find a similar development, with slightly higher values. For 1991 and 2001, we find values of 46.8 and 47.7 percent, respectively. Following this, the IOR decreases until 2005 to 40.6 percent before increasing once again to 45.7 percent in 2008. After 2008, we can identify similar fluctuations in both net and gross earnings. When the effort and income variables of the partner are included, the IOR is fairly constant for net income and is higher than for gross income. Using the full specification, IOR has the same value of 55.2 percent in 1991 and in 2011, with an interim peak of 62.7 in 1997. The observation of a higher IOR in net earnings than in gross earnings can be explained by the lower overall inequality in net earnings, as the IOL remained on a similar level for both gross and net annual income. Our analysis indicates that including partner variables increases IOp by 8.3 (18.0) percent in 1991 and by 23.7 (35.6) percent in 2011 for gross (net) income. The general trend of a decreasing IOR in the baseline case is driven by the increase in earnings inequality rather than by a decrease in IOp.

The IOR for gross annual male earnings shows a generally u-shaped pattern over time. This can be seen in figure 3.4. The baseline case decreases from 36.2 in 1991 to 20.2 percent in 2005. There is a surprising peak of 30.7% in 1999. Between 2008 and 2011, the IOR increased from 20 to 28 percent. We find a similar picture for the other specifications including the spouse's variables, but with higher values. The decrease under the full specification from 1991 to 1998, however, is less marked, declining from 41.9 to 35.8 percent. Only from 2000 to 2009, we find somewhat lower values between 31.1 and 32.9 percent. Thereafter, we once again see an increase to 43.5 percent in 2011.

Figure 3.4 – IOR for annual income - male vs. female



Source: Authors' calculation based on SOEP data

For the female sub-sample, we find significantly lower initial values for the IOR in 1991. The baseline case yields 7 and the full specification 14 percent. The IOR increases over time, regardless of the specification used. As was the case for male earnings, there is a drop in the IOR between 1999 and 2000. Following this, however, the IOR once again increases to 15.6 in 2011 for the baseline case and 32.9 percent for the case of no responsibility.

Comparing male and female IOR for net earnings, the findings are similar to the case of gross earnings, with slightly lower figures. The only difference is in the case of no responsibility. While for men, the income of the spouse yields only limited additional information, IOR for female net earnings increases significantly when the income of the partner is included. IOR is 21.4 in 1991 and increases to 42.5 percent in 1997. IOR decreases to 26.0 in 2002, has a peak in 2008 and then once again decreases again to 36.8 percent in 2011. Adding partner variables to the analysis of gross income increases IOp for men by 15.9 (61.1) percent in 1991 (2011) and for women by 91.2 (111) percent in 1991 (2011).

To sum up, it is clear from our findings that the personal characteristics of the partner play a significant role for IOp. They increase our estimate of the IOR by up to 20 (35) percent for gross (net) earnings. Before analyzing the role of assortative mating in these results in Section 3.4.5, we check the sensitivity of our results with respect to several choices made in the next subsection.

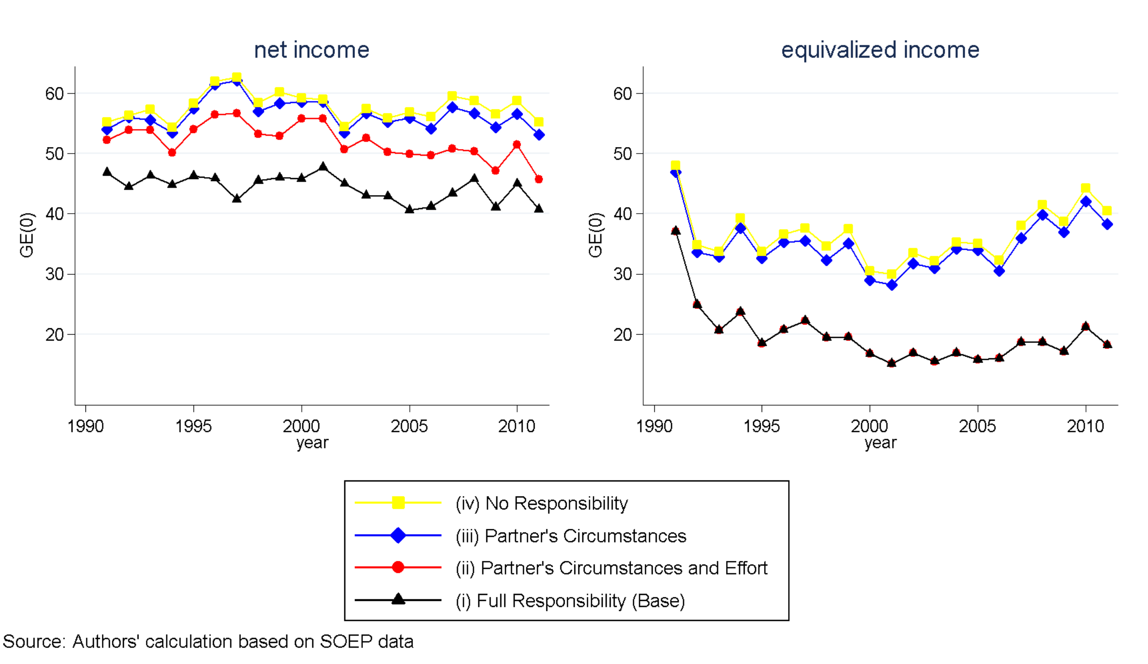
3.4.4 Sensitivity Analysis

Up to this point, our analysis has been limited to individual income using the MLD as our measure of inequality. In this subsection, we aim at overcoming these limitations by broadening our calculations to equivalized household income and applying several other inequality measures apart from the MLD.

In the literature on IOp measurement, individual as well as household income definitions are used. An analysis on an individual level focuses on the potential relationship between the circumstances and effort of the partner and individual outcomes. We do not explicitly control for joint decisions but rather implicitly take them into account by defining scenarios with different degrees of responsibility for the personal characteristics of the partner. It is, however, interesting to follow studies such as Lefranc et al. (2008) and Ferreira and Gignoux (2011) by using equivalized household income as the dependent variable. In this way, we can explore to what extent our results are driven by intra household inequality. However, in order to carry out our analysis using equivalized income, we have to alter the previously defined scenarios to some extent. As equivalized income is identical for all household members, we cannot control for spouse's income lnw_i^P in equations (3.6)–(3.8). Because of this, equation (3.4) becomes identical to (3.6), and scenario (ii) is identical to (i).

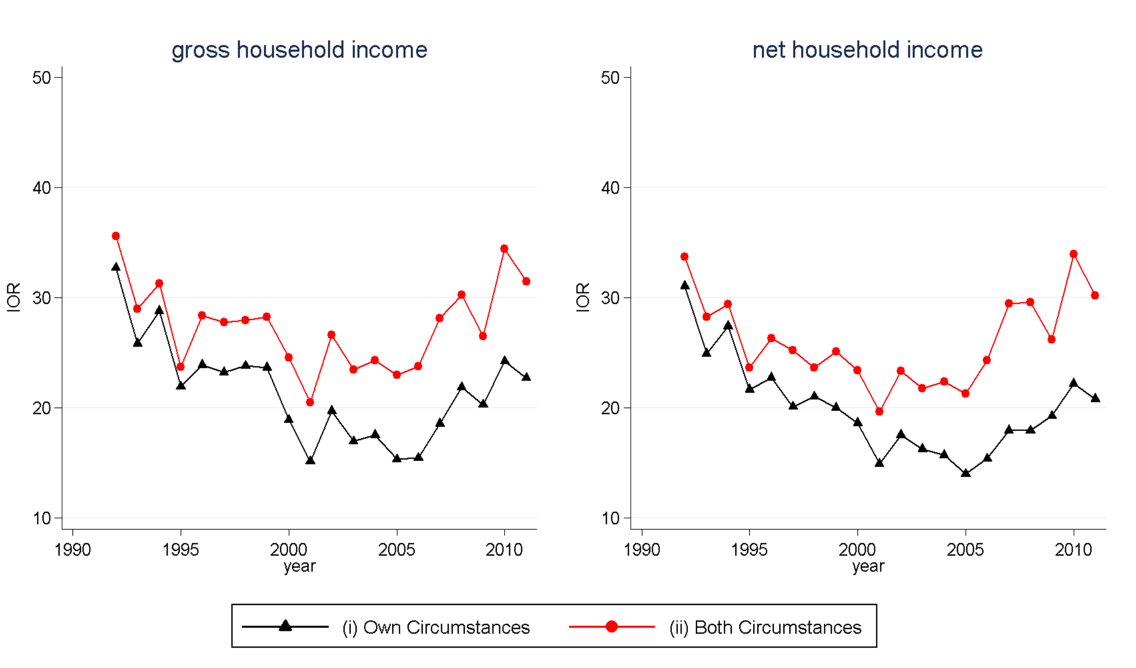
Focusing on the remaining results for equvalized household income in comparison to individual net income in figure 3.5, we find a lower IOR in all specifications for household equivalized income. This is not too surprising as equalizing the income shades potential income differences between spouses, such that the baseline case hovers around 20 percent. However, we do find additional explanatory power fo spouses' variables in the remaining scenarios. This effect is even larger compared to individual income (increase of up to 20 percentage points). Moreover, increases in the IOR are stronger over time in the case of no responsibility compared to the baseline case with equivalized income as well as compared to individual income.

Figure 3.5 – IOR in GE(0) for net equivalized income



Bringing the analysis to the household level, one can also look at joint household income in gross and net terms. We include the circumstances of the household head in the baseline case and add those of the partner as a second scenario. It is a well known fact that inequality between households is lower compared to inequality between individuals, as intra-household inequality accounts for a substantial amount of overall inequality. Therefore, it is less of a surprise that IOp is also lower in the household context. However, it is noteworthy that IOp has a U-shaped pattern and the additional explanatory power of the spouse's circumstances increases over time.

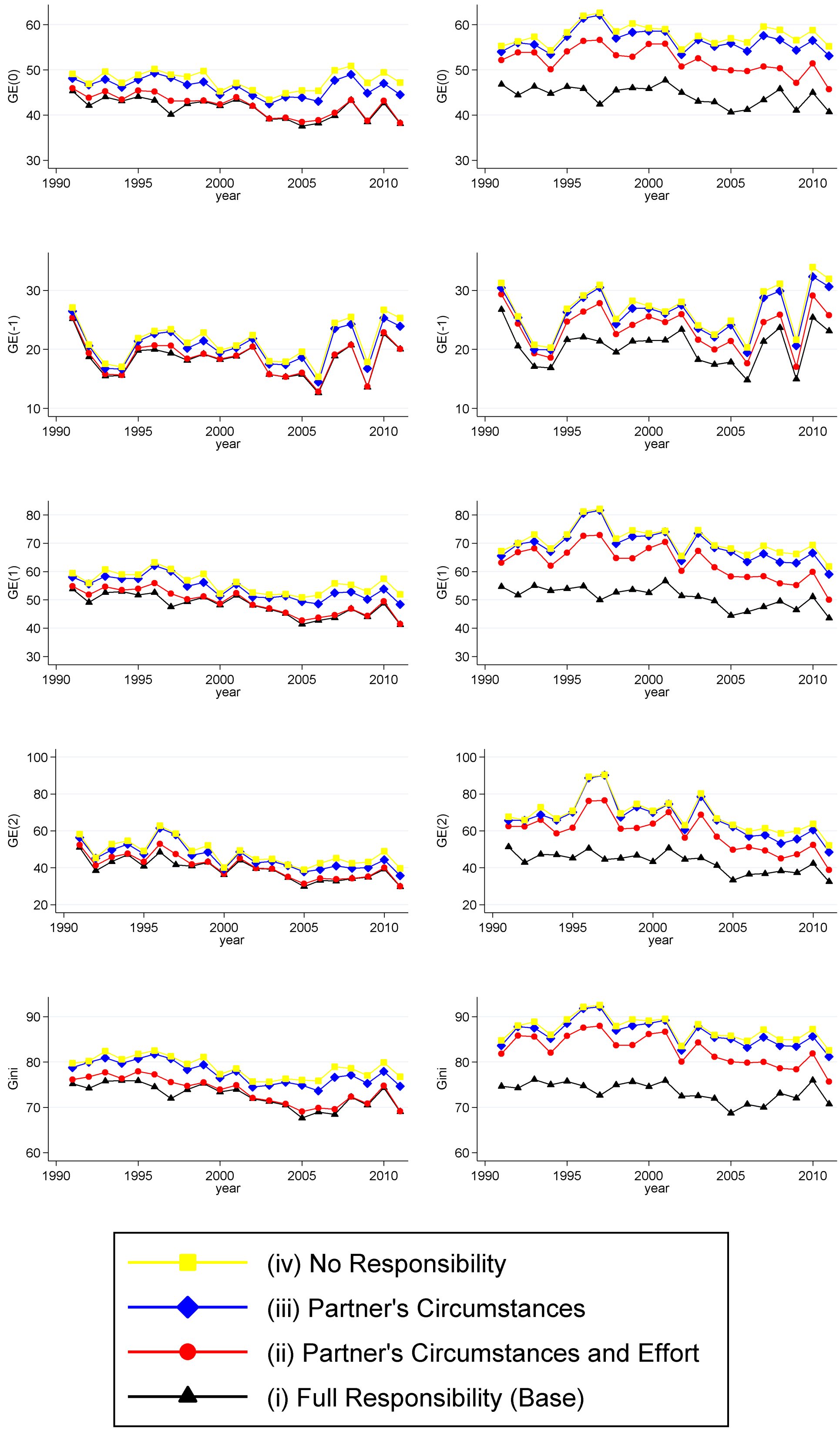
Figure 3.6 – IOR in GE(0) for gross vs. net household income



In the baseline results, calculations were carried out using the MLD as the measure of inequality. This, however, is only one special case from the generalized entropy indices with parameter zero (GE(0)). Pistolesi (2009) for example uses a broad range of inequality measures, including the Gini coefficient. Therefore, we run the analysis using different GE measures with different sensitivity for different parts of the income distribution [GE(-1), G(1), G(2)] as well as the Gini coefficient as inequality measures[14] in order to compare the results both for gross and net income. Generally, the results point in the same direction. In particular, the increased explanatory power of partner variables holds for all inequality measures. Interestingly, overall results for GE(1), GE(2), and the Gini coefficient are higher compared to the MLD (GE(0)), while results for GE(-1) are lower. That is, the IOR is lower when more emphasis is put on the bottom rather than the top of the distribution.

[14]Note that these measures do not fulfill the axiom of path-independent decomposability and hence the results should be interpreted with caution.

Figure 3.7 – IOR for additional measures - gross vs. net



Source: Authors' calculation based on SOEP data

3.4.5 Assortative Mating

In order to gain initial insight into the extent of assortative mating, we compare correlations of key variables displayed in table 3.1. We find a negative correlation between spouses' annual earnings. Considering the differences in part-time work between female and male employees, this is not surprising. The negative correlation decreases over time which might stem from a decline of the male breadwinner model as more women start to work. Looking at hourly wages rather than annual earnings accounts for the difference in labor supply. Here, we find a small but persistent positive correlation in wages. According to the literature Schwartz and Mare (2005), the main variable to measure assortative mating is education. Considering years of education, a positive correlation is found in all years with a range between 0.549 and 0.631. Hence, spouses tend to have similar years of education.

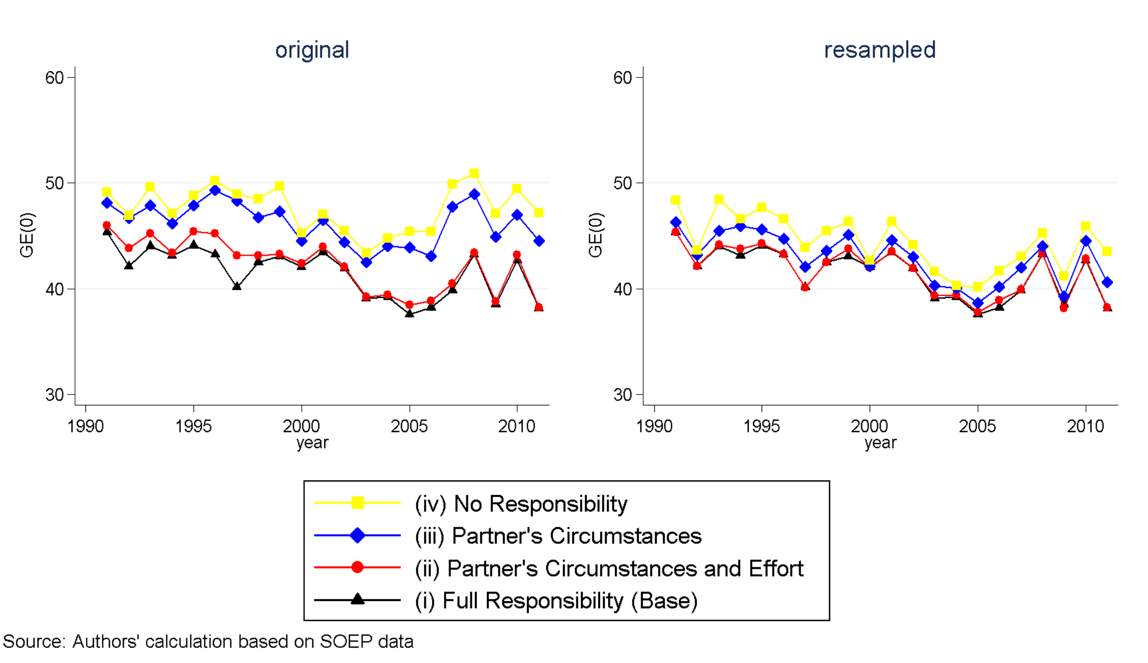
Table 3.1 – Correlations of selected variables

Year	Annual Earnings	Hourly Wages	Education
1992	-0.396	0.080	0.561
1994	-0.368	0.131	0.605
1996	-0.475	0.061	0.572
1998	-0.395	0.119	0.567
2000	-0.391	0.032	0.569
2002	-0.355	0.140	0.563
2004	-0.322	0.070	0.577
2006	-0.338	0.067	0.549
2008	-0.277	0.072	0.593
2010	-0.283	0.076	0.558
2011	-0.225	0.168	0.631

In order to investigate the role of assortative mating for IOp in couples, we follow the literature on assortative mating Burtless (1999); Aaberge et al. (2005); Pestel (2016) and re-compute the IOp measures for the 4 different scenarios by randomly re-matching couples. The effect of assortative mating on inequality is then assessed by comparing the obtained IOp measures for the re-matched sample to the results for the original sample.[15] The resulting IOp measures are displayed in figure 3.8. Note that overall inequality does not change as we do not drop any individuals and the baseline case (without spousal variables) is also not affected. Since, by construction, the correlation between spouses' earnings in the random sample is (close to) zero, the differences between the scenarios disappear. Hence, this exercise shows that assortative mating is

[15]Note that we abstract from potential behavioral responses (such as labor supply) when facing a new partner with different characteristics Pestel (2016). Furthermore, we only compare IOp measures in gross earnings as we would have to re-calculate the total tax burden of the new randomly matched couples in order to also analyze IOp in net earnings.

Figure 3.8 – IOR for gross annual income - rematched couples



key for the conclusion that including a spouse's information in the analysis matters for IOp.

3.5 Conclusion

Thus far, existing literature on IOp has failed to analyze the influence of the partner in measuring IOp of individuals in couples. Using German micro data from 1991 to 2011, we add to the literature by analyzing the effect of a spouse's circumstance, effort and earnings variables on individual earnings and hence IOp. As additional explanatory factors tend to increase lower bounds for IOp, we find that the spouse's variables do in fact have a significant effect on IOp measures. As the effect of spouse's income is declining over time, we suspect that assortative mating is playing a role in this context. The positive correlation between the education level of spouses and a diminishing negative correlation of earnings further emphasize this. Sensitivity analyses using household income concepts as well as further inequality measures apart from MLD suggest that our results are valid not only for the main specification of our analysis but rather hold for a more general case.

To summarize, we find that accounting for personal information of the spouse matters for IOp as our estimate of the IOR increases by up to 20 (35) percent for gross (net) earnings. The less the responsibility assumed for the spouse's variables, the higher the

IOp measure. The question as to which scenario is most suitable cannot be answered without moral judgment and consideration of the context of the research question. Given that spousal variables were completely omitted from analysis in previous studies, the implicit assumption was full responsibility for all spousal variables. It might be questioned whether this omission was intentional or rather inadvertent. Taking into consideration the relationship between spousal earnings via joint income tax filing and different models for decision-making within the household and against the background of increased assortative mating and its potential role in changing inequality, explicitly accounting for the partner (or not) seems to be important when measuring IOp.

Tables and Figures

Variables

Table 3.2 – Variable glossary

Variable	Description
Income	
Individual earnings	Gross and net annual individual earnings
HH equivalized income	OECD equivalence scale
Circumstance Variables	
Gender	Male, Female
Foreign Origin	Nationality: native, foreign
East Germany	Born in East- or West-Germany
Fathers Occupation	Classified: blue collar, farmer, white collar, free-lancer and high qualified, civil servant
Fathers Education	Classified: no degree, secondary, intermediate, upper secondary
Urbanization	Classified: countryside, small city, large city
Year of birth	In years
Height	Body height (in centimeter)
Effort Variables	
Industry	Classified: public administration and social security; fishery, agriculture, forestry; energy, chemicals, steel; engineering; manufacturing; construction; wholesale, trade; transport; financial service; education; health service
Education	In years
Experience	In years, normalized at the age level
Working hours	In weekly hours

Descriptive statistics

Table 3.3 – Descriptive Statistics for Basic Variables

Variable	*Mean*	*Sd*	*Min*	*Max*
Income				
Gross Income	30547.48	26270.32	25	900562.9
Net Income	21832.4	17053.6	22.15	505810.9
Equivalized Income	22021.5	12908.36	2828.89	373132.7
Circumstance Variables				
Female	.5	.5	0	1
Foreign	.04	.21	0	1
East-Germany	.31	.46	0	1
No degree	.01	.11	0	1
Secondary	.76	.43	0	1
Intermediate	.12	.33	0	1
College	.11	.31	0	1
Blue-collar	.55	.5	0	1
Farmer	.04	.19	0	1
White-collar	.14	.35	0	1
Professional	.13	.33	0	1
Self-employed	.07	.25	0	1
Civil servant	.08	.27	0	1
Countryside	.41	.49	0	1
City	.38	.49	0	1
Large City	.21	.41	0	1
Year of birth	1957	7.67	1929	1983
Effort Variables				
Body height	173.22	9.08	139	205
Work Experience	-.06	.93	-3.3	2.71
Weekly working hours	32.47	13.17	.88	93.84
Education	12.7	2.66	7	18
Civil Servant	.1	.3	0	1
Energy and Mining	.13	.34	0	1
Engineering	.07	.26	0	1
Manufacturing	.05	.22	0	1
Construction	.08	.27	0	1
Sales	.15	.35	0	1
Transport	.05	.21	0	1
Financial	.03	.18	0	1
Service	.13	.34	0	1
Education	.1	.29	0	1
Health	.11	.31	0	1

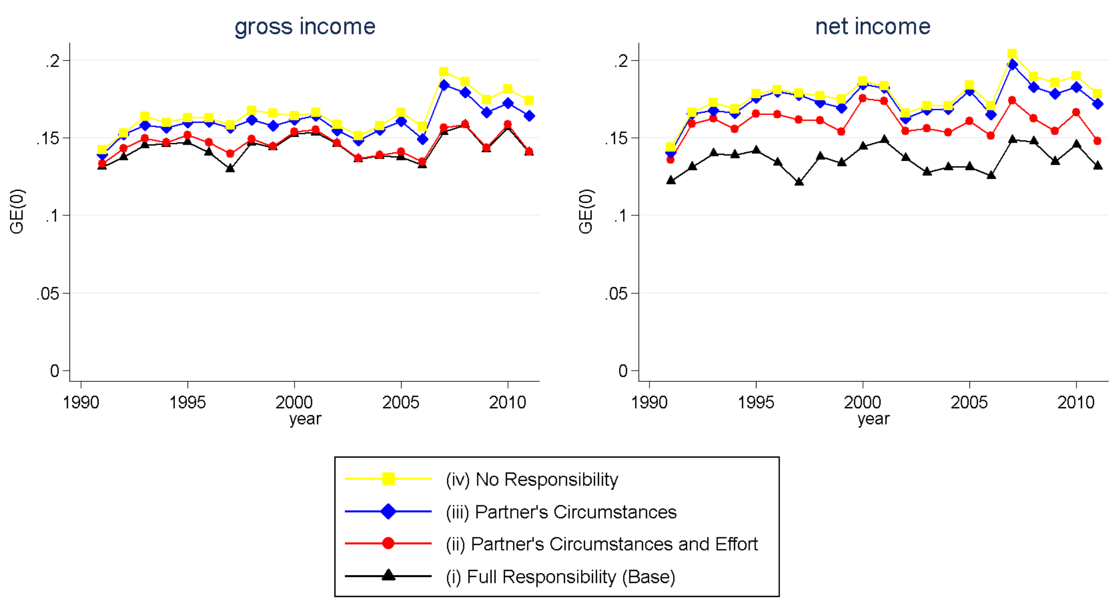
Table 3.4 – Descriptive Statistics for IOp Measures

		IOR Gross Income				IOR Net Income			
Year	*EO*	*Case 1*	*Case 2*	*Case 3*	*Case 4*	*Case 1*	*Case 2*	*Case 3*	*Case 4*
1991	0.29	45.35	45.96	48.14	49.13	46.82	52.19	54.01	55.28
1992	0.33	42.11	43.84	46.65	46.93	44.42	53.87	56.03	56.31
1993	0.33	44.03	45.25	47.90	49.64	46.35	53.88	55.62	57.31
1994	0.34	43.14	43.42	46.19	47.15	44.77	50.15	53.48	54.33
1995	0.33	44.09	45.44	47.87	48.85	46.28	54.06	57.41	58.30
1996	0.32	43.25	45.19	49.36	50.20	45.85	56.43	61.45	61.96
1997	0.32	40.13	43.14	48.32	48.93	42.37	56.67	62.12	62.66
1998	0.35	42.50	43.12	46.75	48.49	45.51	53.26	57.03	58.50
1999	0.33	43.07	43.23	47.33	49.73	45.99	52.90	58.32	60.26
2000	0.36	42.04	42.39	44.51	45.25	45.84	55.75	58.62	59.26
2001	0.35	43.46	43.95	46.49	47.08	47.69	55.82	58.56	58.99
2002	0.35	41.95	42.03	44.38	45.49	45.01	50.72	53.43	54.47
2003	0.35	39.09	39.23	42.50	43.40	43.04	52.55	56.70	57.51
2004	0.35	39.23	39.42	44.02	44.80	42.91	50.30	55.23	55.91
2005	0.37	37.55	38.47	43.89	45.42	40.63	49.87	55.90	56.95
2006	0.35	38.21	38.86	43.05	45.40	41.18	49.72	54.13	56.09
2007	0.39	39.86	40.50	47.72	49.88	43.37	50.77	57.62	59.56
2008	0.37	43.27	43.36	48.97	50.88	45.77	50.35	56.67	58.82
2009	0.37	38.49	38.77	44.87	47.12	41.03	47.10	54.37	56.58
2010	0.37	42.69	43.18	46.99	49.47	45.00	51.41	56.52	58.81
2011	0.37	38.12	38.22	44.52	47.19	40.71	45.70	53.14	55.20

Inequality of opportunity levels

The lower bound level of inequality of opportunity (IOL) is estimated by computing the MLD of the predicted values from the earnings equation. We do this for specifications (3.4), (3.6) – (3.8) using gross and net annual earnings. In addition, we distinguish between male and female individuals. The results for gross and net earnings are shown in figure 3.9. The black line displays the baseline case from equation (3.4). The red line corresponds to the model including spouse's circumstance variables shown by equation (3.6). The case of the model from equation (3.7), including both the spouse's circumstance and effort variables, is displayed by the blue line. Finally, the full specification including circumstances, effort and income variables of the partner corresponds to the yellow graph (equation (3.8)).

Figure 3.9 – IOL for annual income - gross vs. net

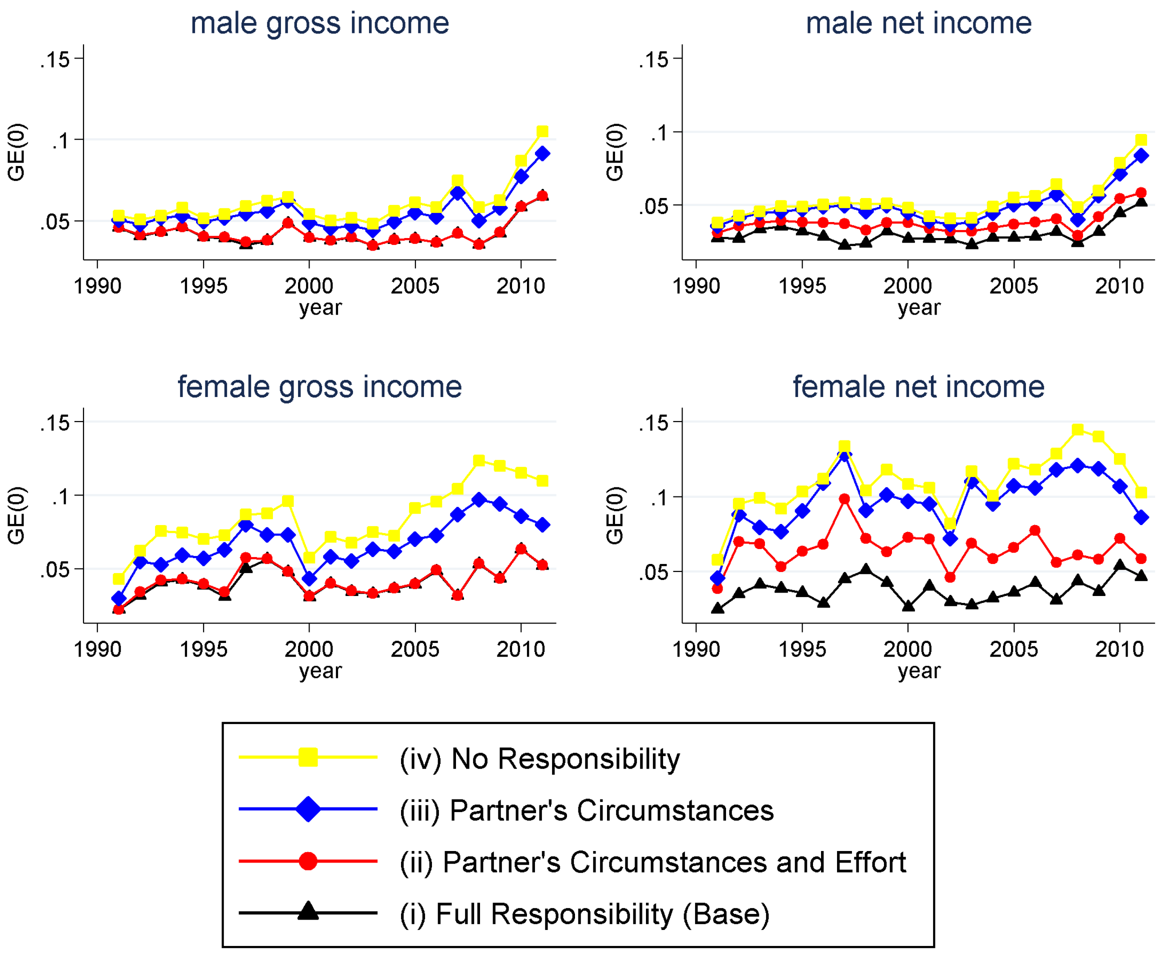


Source: Authors' calculation based on SOEP data

For the baseline case, the inequality of opportunity level is lower for net earnings than for gross earnings. Aside from this, the figures for IOL in net earnings are slightly higher. Regardless of the specification, IOL shows a slightly increasing trend, with a jump between 2006 and 2007. In terms of gross income, controlling for the spouse's income (case of responsibility for spouse's circumstances and effort, displayed by the red graph) does not increase IOL. Using net earnings instead, we find a significant increase in explanatory power and hence also in IOL when controlling for the income of the partner. This indicates that it is an effect of the German tax system, as is already visible from the earnings regression. The effect, however, declines over time, visualized on the graph by the narrowing gap between the red and black lines. This may be due to the changing relation of spouses' incomes. We indeed find a negative, but in absolute terms decreasing, correlation between partners' incomes.[16]

The case of responsibility for spouse's circumstances represented by the blue graph shows mostly identical results for gross and net earnings.[17] Both graphs show an increased IOL compared to the previous cases. Finally, the case of no responsibility, displayed by the yellow graphs, shows a similar development compared to the previous case. However, since 2005, the circumstance variables of the partner increase in explanatory power.

Figure 3.10 – IOL for annual income - male vs. female net



Source: Authors' calculation based on SOEP data

[16]The literature on assortative mating shows an increase in assortative mating in education over time that also reflects in earnings Schwartz and Mare (2005). A detailed discussion on the importance of assortative mating is conducted in 3.4.5.

[17]The case of responsibility for spouse's circumstances is implemented by controlling for spouse's effort and income variables in equation (3.7).

In order to further disentangle the development of IOL, we also consider male and female individuals separately. The results are shown in figure 3.10 for gross and net earnings as well as for male and female sub-samples. In general, we find that the personal information of the partner has more explanatory power for women than for men, thereby increasing our IOL measures in the different scenarios. Male IOL in gross and net earnings is fairly constant until 2008, with a sharp increase thereafter. For the female sub-sample, we find greater fluctuations over time and also higher IOL for all specifications except the baseline case. In contrast to the development of male IOL, the inequality of opportunity level for women decreases after 2008. When comparing male and female sub-samples in terms of net earnings, the case of responsibility for spouse's circumstance (red graph) and effort variables yields interesting results. Controlling for spouse's earnings only slightly increases IOL for men, while there is a significantly larger effect for women. However, following a peak in 1997, the divergence between the black and red graph diminishes over time. This again indicates an effect of the income tax splitting as well as changing correlation in spouses' earnings.

Appendix: Accounting for the Spouse when Measuring Inequality of Opportunity.

Table 3.5 – Regression Results Baseline Case - Gross Income (1991 - 2000)

Variables	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Female	-0.854***	-0.984***	-0.934***	-0.939***	-1.007***	-0.917***	-0.859***	-0.775***	-0.809***	-0.951***
	-0.06	-0.066	-0.069	-0.07	-0.068	-0.069	-0.07	-0.068	-0.066	-0.05
Foreign origin	0.17	0.159	0.087	0.163	-0.021	-0.072	0.063	-0.218*	-0.609***	-0.500***
	-0.112	-0.132	-0.139	-0.144	-0.15	-0.139	-0.139	-0.129	-0.129	-0.074
Region (East/ South)	-0.280***	-0.140***	-0.089	-0.116**	-0.113**	-0.136**	-0.094*	-0.031	-0.157***	-0.157***
	-0.047	-0.054	-0.056	-0.056	-0.055	-0.056	-0.054	-0.054	-0.05	-0.043
Secondary	-0.209	0.002	-0.214	1.421**	0.53	-0.332	-0.398	-0.337	-0.715**	-0.264*
	-1.541	-1.734	-1.685	-0.661	-0.642	-1.781	-0.563	-0.331	-0.338	-0.136
Intermediate	-0.15	0.286	-0.063	1.525**	0.744	-0.188	-0.204	-0.157	-0.463	-0.161
	-1.543	-1.736	-1.687	-0.666	-0.647	-1.783	-0.568	-0.339	-0.345	-0.144
College	-0.152	0.32	0.002	1.596**	0.822	-0.13	-0.185	-0.353	-0.572*	-0.225
	-1.544	-1.737	-1.688	-0.669	-0.649	-1.784	-0.571	-0.342	-0.348	-0.151
Farmer	0.118	0.134	0.235*	0.157	-0.024	-0.004	0.026	0.098	-0.05	0.112
	-0.121	-0.123	-0.131	-0.138	-0.122	-0.123	-0.124	-0.12	-0.123	-0.092
White-collar	0.104	0.045	0.032	0.140*	0.064	0.128*	0.019	-0.033	0.031	0.007
	-0.063	-0.071	-0.073	-0.075	-0.072	-0.072	-0.074	-0.07	-0.069	-0.055
Professional	0.181**	-0.121	-0.145	0.059	-0.046	0.081	0.021	0.021	0.065	0.203***
	-0.078	-0.085	-0.09	-0.092	-0.085	-0.087	-0.085	-0.083	-0.078	-0.068
Self-employed	0.145*	0.075	0.046	-0.033	0.012	0.107	0.07	-0.014	-0.103	-0.017
	-0.08	-0.096	-0.101	-0.101	-0.1	-0.102	-0.105	-0.105	-0.104	-0.076
Civil servant	0.027	-0.057	0.118	0.162	0.071	0.032	0.158	0.254***	0.198**	0.126*
	-0.085	-0.093	-0.096	-0.103	-0.099	-0.102	-0.1	-0.091	-0.092	-0.07
City	0.083*	0.041	0.109*	0.145**	0.123**	0.073	0.142**	0.125**	0.101*	0.115***
	-0.047	-0.054	-0.056	-0.057	-0.055	-0.055	-0.056	-0.053	-0.053	-0.04
Large City	0.110**	0.133**	0.142**	0.167**	0.126**	0.059	0.168***	0.183***	0.159**	0.182***
	-0.055	-0.062	-0.063	-0.066	-0.064	-0.065	-0.065	-0.065	-0.063	-0.05
birth	-0.011***	-0.011***	-0.015***	-0.015***	-0.014***	-0.017***	-0.020***	-0.023***	-0.017***	-0.015***
	-0.003	-0.003	-0.003	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.003
height	0.007**	-0.001	0.005	0.004	0	0.005	0.005	0.014***	0.011***	0.004
	-0.003	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.003
Constant	30.405***	32.379***	38.495***	38.055***	36.332***	43.805***	48.427***	53.819***	41.599***	40.134***
	-5.509	-6.401	-6.809	-7.097	-6.887	-7.137	-7.276	-7.047	-6.807	-5.233
Observations	1,182	1,162	1,130	1,124	1,124	1,092	1,076	1,202	1,210	2,146
R-squared	0.365	0.325	0.33	0.321	0.341	0.334	0.316	0.334	0.335	0.331

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.6 – Regression Results Baseline Case - Gross Income (2001 - 2011)

Variables	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Female	-1.039***	-0.931***	-0.857***	-0.881***	-0.901***	-0.751***	-0.954***	-0.793***	-0.809***	-0.778***	-0.669***
	-0.05	-0.046	-0.049	-0.051	-0.055	-0.053	-0.057	-0.057	-0.065	-0.065	(0.074)
Foreign origin	-0.267***	-0.274***	-0.130**	-0.149**	-0.197***	-0.243***	-0.271***	-0.338***	-0.337***	-0.480***	-0.299***
	-0.067	-0.059	-0.062	-0.064	-0.068	-0.063	-0.07	-0.064	-0.075	-0.075	(0.091)
Region (East/ South)	-0.165***	-0.180***	-0.158***	-0.178***	-0.152***	-0.07	-0.129***	-0.130***	-0.223***	-0.214***	-0.119*
	-0.041	-0.038	-0.043	-0.044	-0.046	-0.046	-0.049	-0.05	-0.057	-0.056	(0.065)
Secondary	-0.219*	0.049	0.202*	0.245**	0.059	-0.146	-0.360***	-0.482***	-0.542***	-0.496***	-0.649***
	-0.127	-0.113	-0.106	-0.103	-0.104	-0.102	-0.106	-0.106	-0.126	-0.142	(0.152)
Intermediate	-0.219	-0.053	0.123	0.112	-0.15	-0.408***	-0.334***	-0.444***	-0.603***	-0.503***	-0.621***
	-0.136	-0.121	-0.117	-0.114	-0.115	-0.114	-0.12	-0.12	-0.139	-0.154	(0.167)
College	-0.159	0.191	0.340***	0.270**	0.018	-0.109	-0.15	-0.366***	-0.337**	-0.253	-0.349**
	-0.146	-0.129	-0.126	-0.123	-0.125	-0.122	-0.128	-0.125	-0.148	-0.164	(0.175)
Farmer	-0.001	-0.150*	-0.02	-0.121	0.01	0.055	0.097	-0.148	0.024	-0.280**	0.207
	-0.096	-0.085	-0.093	-0.097	-0.101	-0.097	-0.1	-0.105	-0.114	-0.125	(0.135)
White-collar	-0.047	-0.02	0.059	-0.028	-0.009	0.086	0.035	0.141**	0.154**	0.011	-0.089
	-0.055	-0.049	-0.055	-0.056	-0.06	-0.056	-0.061	-0.058	-0.068	-0.067	(0.076)
Professional	0.173**	0.228***	0.148**	0.341***	0.236***	0.255***	0.153**	0.086	0.234***	0.212**	0.062
	-0.068	-0.062	-0.068	-0.066	-0.068	-0.067	-0.073	-0.072	-0.08	-0.085	(0.093)
Self-employed	-0.144*	-0.081	-0.184**	-0.025	0.132	0.047	-0.082	0.019	0.150*	0.239**	0.049
	-0.076	-0.066	-0.073	-0.073	-0.082	-0.076	-0.08	-0.074	-0.09	-0.093	(0.106)
Civil servant	0.374***	0.269***	0.196***	0.161**	0.185**	0.263***	0.154*	0.311***	0.186**	0.303***	0.200*
	-0.075	-0.067	-0.072	-0.07	-0.076	-0.074	-0.079	-0.079	-0.09	-0.094	(0.102)
City	0.063	0.080**	0.033	0.104***	0.179***	0.084**	0.034	0.01	0.057	0.036	0.046
	-0.04	-0.037	-0.04	-0.04	-0.043	-0.042	-0.045	-0.044	-0.051	-0.051	(0.058)
Large City	0.134***	0.130***	0.159***	0.172***	0.189***	0.155***	0.046	0.092*	0.147**	0.095	0.198***
	-0.049	-0.045	-0.048	-0.049	-0.051	-0.05	-0.053	-0.055	-0.063	-0.065	(0.073)
birth	-0.009***	-0.013***	-0.013***	-0.008***	-0.007**	-0.009***	-0.008**	-0.018***	-0.004	-0.012***	-0.002
	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.004	-0.004	(0.005)
height	0	0.004*	0.008***	0.006**	0.005*	0.012***	0.007**	0.014***	0.011***	0.012***	0.017***
	-0.003	-0.002	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	(0.004)
Constant	28.687***	35.016***	33.300***	25.047***	23.857***	26.721***	25.254***	43.825***	16.981**	32.119***	12.037
	-5.199	-4.854	-5.344	-5.625	-6.4	-6.096	-6.531	-6.426	-7.649	-7.694	(9.210)
Observations	2,136	2,361	2,273	2,148	1,891	1,959	1,825	1,607	1,413	1,239	970
R-squared	0.337	0.341	0.302	0.308	0.31	0.309	0.331	0.367	0.302	0.349	0.322

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7 – Regression Results Baseline Case - Net Income (1991 - 2000)

Variables	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Female	-0.855***	-0.979***	-0.942***	-0.952***	-1.005***	-0.938***	-0.879***	-0.831***	-0.861***	-0.985***
	-0.056	-0.062	-0.065	-0.066	-0.064	-0.064	-0.065	-0.063	-0.06	-0.046
Foreign	0.174*	0.16	0.09	0.194	-0.059	-0.046	0.118	-0.259**	-0.632***	-0.349***
	-0.104	-0.123	-0.131	-0.134	-0.141	-0.129	-0.129	-0.12	-0.117	-0.067
Region	-0.122***	-0.021	-0.009	-0.053	-0.062	-0.073	-0.019	0.039	-0.076*	-0.069*
	-0.044	-0.05	-0.052	-0.052	-0.051	-0.052	-0.05	-0.05	-0.046	-0.039
Secondary	-0.205	-0.035	-0.215	1.088*	-0.006	-0.312	-0.421	-0.16	-0.514*	-0.235*
	-1.439	-1.618	-1.582	-0.616	-0.602	-1.654	-0.522	-0.306	-0.308	-0.123
Intermediate	-0.158	0.204	-0.095	1.160*	0.168	-0.207	-0.251	0.005	-0.294	-0.167
	-1.441	-1.621	-1.585	-0.62	-0.607	-1.655	-0.527	-0.314	-0.315	-0.13
College	-0.126	0.27	0.005	1.257**	0.268	-0.116	-0.256	-0.161	-0.381	-0.174
	-1.442	-1.621	-1.585	-0.623	-0.609	-1.656	-0.529	-0.317	-0.317	-0.136
Farmer	0.157	0.182	0.266**	0.18	0.048	0.038	0.062	0.123	0.024	0.140*
	-0.113	-0.115	-0.123	-0.129	-0.114	-0.114	-0.115	-0.111	-0.112	-0.083
White-collar	0.102*	0.05	0.041	0.132*	0.062	0.120*	0.019	-0.029	0.03	0.033
	-0.059	-0.067	-0.069	-0.07	-0.067	-0.067	-0.069	-0.065	-0.063	-0.049
Professional	0.167**	-0.095	-0.126	0.055	-0.024	0.069	0.004	-0.021	0.043	0.182***
	-0.073	-0.079	-0.084	-0.086	-0.08	-0.08	-0.079	-0.077	-0.071	-0.062
Self-employed	0.151**	0.092	0.067	-0.009	0.04	0.136	0.111	0.02	-0.052	0.015
	-0.075	-0.09	-0.094	-0.094	-0.094	-0.094	-0.098	-0.097	-0.094	-0.069
Civil	0.03	-0.038	0.119	0.161*	0.07	0.035	0.172*	0.217***	0.190**	0.162**
	-0.079	-0.087	-0.091	-0.096	-0.093	-0.095	-0.093	-0.084	-0.084	-0.064
City	0.076*	0.039	0.087	0.118**	0.115**	0.048	0.114**	0.086*	0.061	0.077**
	-0.044	-0.05	-0.053	-0.053	-0.051	-0.051	-0.052	-0.049	-0.048	-0.036
Large	0.092*	0.118**	0.113*	0.133**	0.101*	0.018	0.115*	0.161***	0.114**	0.149***
	-0.051	-0.058	-0.06	-0.062	-0.06	-0.06	-0.06	-0.06	-0.057	-0.045
birth	-0.008***	-0.008***	-0.011***	-0.011***	-0.010***	-0.012***	-0.013***	-0.017***	-0.011***	-0.012***
	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.002
height	0.008**	0	0.005	0.004	0.001	0.004	0.004	0.010***	0.008**	0.003
	-0.003	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.003	-0.002
Constant	24.457***	25.054***	30.701***	29.817***	29.512***	33.825***	35.784***	41.538***	30.211***	32.353***
	-5.143	-5.975	-6.395	-6.61	-6.461	-6.626	-6.747	-6.516	-6.204	-4.721
Observations	1182	1162	1130	1124	1124	1092	1076	1202	1210	2146
R-squared	0.38	0.346	0.35	0.341	0.362	0.357	0.334	0.357	0.363	0.365

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.8 – Regression Results Baseline Case - Net Income (2001 - 2011)

Variables	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Female	-1.063***	-0.956***	-0.881***	-0.917***	-0.921***	-0.774***	-0.985***	-0.832***	-0.828***	-0.801***	-0.713***
	-0.046	-0.042	-0.044	-0.046	-0.051	-0.049	-0.052	-0.053	-0.06	-0.06	-0.068
Foreign	-0.234***	-0.206***	-0.08	-0.105*	-0.163***	-0.202***	-0.203***	-0.252***	-0.266***	-0.404***	-0.244***
	-0.062	-0.054	-0.056	-0.058	-0.062	-0.058	-0.065	-0.059	-0.069	-0.069	-0.083
Region(Ost/West)	-0.067*	-0.092***	-0.071*	-0.099**	-0.063	-0.022	-0.069	-0.085*	-0.158***	-0.161***	-0.09
	-0.038	-0.035	-0.038	-0.04	-0.043	-0.042	-0.045	-0.046	-0.052	-0.052	-0.059
Secondary	-0.265**	0.003	0.019	0.12	0.006	-0.114	-0.309***	-0.399***	-0.497***	-0.412***	-0.560***
	-0.116	-0.103	-0.095	-0.094	-0.095	-0.094	-0.097	-0.098	-0.116	-0.131	-0.139
Intermediate	-0.251**	-0.047	-0.029	0.001	-0.149	-0.336***	-0.300***	-0.376***	-0.572***	-0.437***	-0.552***
	-0.124	-0.111	-0.105	-0.104	-0.106	-0.104	-0.11	-0.111	-0.128	-0.141	-0.152
College	-0.193	0.127	0.142	0.167	-0.015	-0.067	-0.14	-0.341***	-0.334**	-0.224	-0.322**
	-0.133	-0.118	-0.113	-0.112	-0.115	-0.112	-0.118	-0.115	-0.137	-0.151	-0.16
Farmer	0.05	-0.056	0.052	-0.065	0.08	0.081	0.104	-0.039	0.097	-0.257**	0.225*
	-0.088	-0.077	-0.084	-0.088	-0.093	-0.089	-0.092	-0.097	-0.105	-0.115	-0.124
White-collar	-0.046	-0.018	0.057	-0.036	-0.013	0.085*	0.029	0.129**	0.129**	-0.01	-0.084
	-0.05	-0.045	-0.049	-0.051	-0.055	-0.052	-0.056	-0.054	-0.063	-0.061	-0.07
Professional	0.159**	0.197***	0.114*	0.298***	0.212***	0.228***	0.155**	0.096	0.223***	0.202***	0.058
	-0.062	-0.057	-0.061	-0.061	-0.063	-0.061	-0.067	-0.066	-0.074	-0.078	-0.085
Self-employed	-0.112	-0.05	-0.151**	-0.029	0.128*	0.017	-0.061	0.032	0.157*	0.213**	0.037
	-0.07	-0.06	-0.065	-0.067	-0.076	-0.07	-0.074	-0.068	-0.083	-0.085	-0.097
Civil-Servant	0.352***	0.246***	0.152**	0.147**	0.198***	0.234***	0.149**	0.281***	0.178**	0.279***	0.230**
	-0.069	-0.061	-0.065	-0.064	-0.069	-0.068	-0.073	-0.073	-0.083	-0.087	-0.093
City	0.026	0.051	0.011	0.065*	0.128***	0.049	0.004	-0.008	0.045	0.022	0.037
	-0.037	-0.034	-0.036	-0.037	-0.04	-0.038	-0.041	-0.041	-0.047	-0.047	-0.053
Large-City	0.098**	0.094**	0.127***	0.128***	0.114**	0.106**	0.014	0.094*	0.130**	0.087	0.180***
	-0.045	-0.041	-0.043	-0.045	-0.047	-0.046	-0.049	-0.05	-0.058	-0.06	-0.067
birth	-0.006**	-0.008***	-0.008***	-0.003	-0.003	-0.005*	-0.004	-0.013***	-0.001	-0.007**	0.003
	-0.002	-0.002	-0.002	-0.003	-0.003	-0.003	-0.003	-0.003	-0.004	-0.004	-0.004
height	-0.001	0.004	0.007***	0.005*	0.005*	0.011***	0.006**	0.013***	0.010***	0.011***	0.014***
	-0.002	-0.002	-0.002	-0.002	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.004
Constant	22.221***	25.571***	24.940***	15.561***	14.708**	18.889***	17.204***	33.082***	11.556	23.056***	2.096
	-4.764	-4.432	-4.801	-5.125	-5.875	-5.594	-6.025	-5.928	-7.072	-7.084	-8.418
Observations	2,136	2,361	2,273	2,148	1,891	1,959	1,825	1,607	1,413	1,239	970
R-squared	0.37	0.368	0.335	0.337	0.337	0.335	0.36	0.389	0.324	0.371	0.35

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.9 – Regression Results Full Responsibility - Gross Income (1999-2000)

Variables	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Female	-0.666***	-0.787***	-0.732***	-0.856***	-0.803***	-0.718***	-0.708***	-0.706***	-0.910***	-0.887***
	(0.094)	(0.104)	(0.107)	(0.112)	(0.108)	(0.108)	(0.108)	(0.108)	(0.106)	(0.080)
Foreign origin	0.132	0.070	0.048	0.065	-0.084	-0.232	-0.088	-0.302**	-0.704***	-0.470***
	(0.116)	(0.138)	(0.143)	(0.150)	(0.154)	(0.145)	(0.144)	(0.130)	(0.129)	(0.098)
Region (East/ South)	-0.221***	-0.397	-0.846*	-0.564*	-0.374	-0.621*	-0.233	-0.214	-0.152	-0.412***
	(0.051)	(0.635)	(0.446)	(0.315)	(0.440)	(0.318)	(0.289)	(0.214)	(0.212)	(0.153)
Secondary	-0.288	0.033	-0.523	0.856	0.694	-0.447	0.230	-0.205	-0.414	-0.258
	-1.536	-1.724	-1.670	-1.364	-1.294	-1.762	-1.211	(0.359)	(0.376)	(0.163)
Intermediate	-0.262	0.281	-0.385	0.908	0.880	-0.347	0.401	-0.081	-0.206	-0.186
	-1.538	-1.727	-1.673	-1.367	-1.297	-1.764	-1.214	(0.362)	(0.378)	(0.171)
College	-0.241	0.365	-0.314	0.980	0.971	-0.294	0.420	-0.249	-0.358	-0.284
	-1.539	-1.727	-1.673	-1.368	-1.297	-1.765	-1.215	(0.371)	(0.386)	(0.178)
Farmer	0.084	0.106	0.206	0.134	-0.021	0.035	-0.033	0.064	-0.011	0.136
	(0.122)	(0.123)	(0.131)	(0.140)	(0.123)	(0.123)	(0.124)	(0.119)	(0.122)	(0.093)
White-collar	0.086	0.015	0.010	0.123	0.056	0.116	-0.009	-0.075	-0.013	-0.012
	(0.065)	(0.073)	(0.074)	(0.078)	(0.073)	(0.073)	(0.074)	(0.071)	(0.069)	(0.055)
Professional	0.168**	-0.105	-0.165*	-0.016	-0.078	0.048	-0.013	-0.010	0.014	0.153**
	(0.080)	(0.088)	(0.093)	(0.095)	(0.089)	(0.089)	(0.088)	(0.084)	(0.079)	(0.069)
Self-employed	0.116	0.053	0.059	-0.035	0.038	0.133	0.036	-0.062	-0.180*	-0.030
	(0.082)	(0.097)	(0.101)	(0.103)	(0.101)	(0.103)	(0.107)	(0.104)	(0.102)	(0.076)
Civil servant	0.020	-0.070	0.130	0.127	0.057	-0.032	0.077	0.208**	0.139	0.096
	(0.087)	(0.096)	(0.098)	(0.105)	(0.101)	(0.103)	(0.101)	(0.091)	(0.091)	(0.071)
City	0.065	0.028	0.085	0.134**	0.123**	0.053	0.160***	0.143***	0.082	0.072*
	(0.049)	(0.056)	(0.058)	(0.060)	(0.057)	(0.056)	(0.057)	(0.055)	(0.054)	(0.043)
Large City	0.054	0.086	0.110	0.162**	0.127*	0.019	0.151**	0.223***	0.148**	0.164***
	(0.065)	(0.071)	(0.072)	(0.076)	(0.073)	(0.072)	(0.072)	(0.071)	(0.069)	(0.056)
birth	-0.017***	-0.013*	-0.027***	-0.020***	-0.027***	-0.023***	-0.022***	-0.020***	-0.017**	-0.016***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)
height	0.007**	-0.001	0.005	0.004	0.001	0.004	0.003	0.011***	0.006	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Partner Variables										
Region (East/ South)		0.310	0.858*	0.493	0.318	0.546*	0.188	0.228	-0.006	0.287*
		(0.635)	(0.445)	(0.315)	(0.439)	(0.318)	(0.290)	(0.215)	(0.213)	(0.153)
Foreign origin	-0.028	0.085	-0.106	-0.054	0.064	0.107	-0.036	0.038	0.105	-0.049
	(0.122)	(0.144)	(0.151)	(0.158)	(0.163)	(0.152)	(0.148)	(0.138)	(0.139)	(0.100)
Secondary	-0.735	-0.653	-0.944	0.476	-0.247	-1.270	-0.764	-0.724*	-0.614	-0.092
	-1.588	-1.781	-1.735	-1.373	-1.303	-1.850	-1.218	(0.375)	(0.382)	(0.162)
Intermediate	-0.783	-0.561	-1.025	0.534	-0.308	-1.224	-0.836	-0.910**	-0.762**	-0.086
	-1.590	-1.784	-1.738	-1.376	-1.306	-1.852	-1.220	(0.379)	(0.385)	(0.170)
College	-0.674	-0.675	-1.209	0.489	-0.353	-1.347	-0.767	-0.766**	-0.855**	-0.256
	-1.591	-1.786	-1.739	-1.378	-1.307	-1.853	-1.221	(0.386)	(0.392)	(0.178)
Farmer	0.059	-0.102	-0.083	0.070	0.030	0.051	-0.078	-0.211*	-0.366***	-0.161*
	(0.121)	(0.123)	(0.134)	(0.144)	(0.124)	(0.126)	(0.124)	(0.120)	(0.122)	(0.094)
White-Collar	0.053	0.002	0.011	0.102	0.101	0.003	0.108	0.116*	0.123*	0.026
	(0.065)	(0.073)	(0.074)	(0.076)	(0.073)	(0.073)	(0.074)	(0.070)	(0.070)	(0.056)
Professional	0.153*	0.037	0.129	0.126	0.060	-0.042	0.112	0.212**	0.213***	0.076
	(0.080)	(0.088)	(0.093)	(0.095)	(0.089)	(0.088)	(0.087)	(0.085)	(0.079)	(0.070)
Self-employed	0.140*	-0.018	0.143	0.148	0.152	-0.150	0.009	0.087	-0.008	-0.020
	(0.080)	(0.096)	(0.100)	(0.101)	(0.102)	(0.102)	(0.106)	(0.103)	(0.101)	(0.077)
Civil servant	0.041	0.008	0.178*	0.018	0.149	0.080	0.130	-0.087	-0.006	0.059
	(0.086)	(0.095)	(0.098)	(0.105)	(0.101)	(0.103)	(0.102)	(0.092)	(0.091)	(0.071)
City	0.065	-0.035	0.031	0.005	0.014	0.034	-0.040	-0.067	0.028	0.060
	(0.049)	(0.055)	(0.058)	(0.060)	(0.057)	(0.057)	(0.058)	(0.055)	(0.054)	(0.043)
Large city	0.094	0.032	0.080	0.022	0.045	0.111	0.038	-0.082	0.016	-0.004
	(0.065)	(0.071)	(0.072)	(0.076)	(0.073)	(0.073)	(0.072)	(0.072)	(0.069)	(0.056)
birth	0.008	0.001	0.013*	0.006	0.018**	0.007	0.004	-0.003	0.001	-0.001
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)
height	-0.001	0.000	0.003	0.004	-0.002	0.001	0.002	0.005	0.010**	0.005*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Experience	-0.074**	-0.065*	-0.045	-0.075**	-0.027	-0.039	-0.058	-0.016	-0.050	-0.076***
	(0.029)	(0.033)	(0.035)	(0.037)	(0.037)	(0.038)	(0.039)	(0.037)	(0.037)	(0.028)
Sq experience	-0.010	-0.021	0.001	0.025	0.028	0.017	0.035	0.027	0.006	-0.019
	(0.019)	(0.023)	(0.023)	(0.025)	(0.025)	(0.026)	(0.026)	(0.023)	(0.023)	(0.018)
Weekly hours	-0.005**	-0.010***	-0.010***	-0.002	-0.003	-0.008***	-0.010***	-0.007***	-0.003	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Education	-0.014	-0.013	0.010	0.015	0.002	0.020	0.009	0.013	0.046***	0.028***
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.011)	(0.009)
Energy and Mining	-0.121	-0.007	-0.051	-0.141	-0.032	-0.066	-0.017	-0.102	-0.105	0.062
	(0.084)	(0.093)	(0.096)	(0.102)	(0.096)	(0.099)	(0.100)	(0.097)	(0.098)	(0.076)
Engineering	-0.137	-0.098	-0.081	-0.067	-0.010	-0.083	-0.115	0.086	-0.060	0.096
	(0.100)	(0.114)	(0.120)	(0.115)	(0.111)	(0.121)	(0.121)	(0.118)	(0.110)	(0.088)
Manufacturing	0.005	0.116	0.002	-0.088	-0.047	-0.003	-0.205	0.192	0.096	0.008
	(0.103)	(0.120)	(0.123)	(0.127)	(0.126)	(0.125)	(0.125)	(0.121)	(0.123)	(0.093)
Construction	-0.018	0.006	-0.080	-0.199*	0.056	-0.093	0.039	-0.089	-0.014	0.008
	(0.101)	(0.110)	(0.113)	(0.113)	(0.110)	(0.109)	(0.118)	(0.120)	(0.110)	(0.085)
Sales	-0.120	-0.067	-0.172*	-0.078	-0.055	-0.062	-0.140	-0.042	-0.110	-0.034
	(0.086)	(0.094)	(0.097)	(0.099)	(0.095)	(0.098)	(0.098)	(0.095)	(0.095)	(0.072)
Transport	-0.151	-0.202	-0.184	-0.211	-0.366***	-0.222*	0.015	-0.169	-0.239*	-0.026
	(0.115)	(0.125)	(0.137)	(0.137)	(0.137)	(0.134)	(0.138)	(0.135)	(0.133)	(0.098)
Financial	0.085	0.159	0.121	0.036	0.164	0.373**	0.341**	0.402***	0.087	0.134
	(0.130)	(0.143)	(0.145)	(0.147)	(0.144)	(0.152)	(0.154)	(0.142)	(0.145)	(0.130)
Service	-0.010	0.088	-0.008	-0.275***	0.038	0.061	0.101	0.235**	0.154*	0.043
	(0.100)	(0.104)	(0.106)	(0.106)	(0.105)	(0.108)	(0.102)	(0.096)	(0.093)	(0.072)
Education	0.021	0.088	-0.127	-0.128	0.159	0.154	0.080	0.064	-0.106	0.037
	(0.107)	(0.112)	(0.113)	(0.113)	(0.110)	(0.116)	(0.121)	(0.112)	(0.111)	(0.084)
Health	-0.123	-0.124	-0.133	-0.162	-0.076	-0.051	-0.205**	0.011	-0.065	0.104
	(0.099)	(0.103)	(0.108)	(0.112)	(0.105)	(0.103)	(0.102)	(0.098)	(0.100)	(0.079)
Constant	28.294***	32.917***	37.227***	33.739***	27.994***	40.602***	45.181***	51.498***	38.703***	42.327***
Observations	1,182	1,162	1,13	1,124	1,124	1,092	1,076	1,202	1,21	2,146
R-squared	0.389	0.354	0.365	0.345	0.365	0.371	0.359	0.379	0.386	0.355

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.10 – Regression Results Full Responsibility - Gross Income (2001-2011)

Variables	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Female	-0.969***	-0.875***	-0.769***	-0.860***	-0.756***	-0.601***	-0.698***	-0.682***	-0.699***	-0.709***	-0.660***
	(0.080)	(0.073)	(0.076)	(0.079)	(0.084)	(0.080)	(0.083)	(0.084)	(0.097)	(0.102)	(0.112)
Foreign origin	-0.312***	-0.297***	-0.174**	-0.197**	-0.247***	-0.337***	-0.285***	-0.404***	-0.424***	-0.566***	-0.346***
	(0.087)	(0.075)	(0.082)	(0.084)	(0.082)	(0.073)	(0.081)	(0.082)	(0.095)	(0.098)	(0.123)
Region (East/ South)	-0.281**	-0.188	-0.112	0.188	-0.055	0.079	0.175	-0.807***	-0.250	-0.349**	-0.253
	(0.142)	(0.143)	(0.184)	(0.170)	(0.179)	(0.165)	(0.166)	(0.164)	(0.178)	(0.177)	(0.286)
Secondary	-0.280*	-0.003	0.161	0.081	0.024	-0.155	-0.344***	-0.501***	-0.661***	-0.471***	-0.673***
	(0.149)	(0.127)	(0.119)	(0.123)	(0.116)	(0.107)	(0.107)	(0.109)	(0.130)	(0.148)	(0.159)
Intermediate	-0.315**	-0.143	0.041	-0.076	-0.248**	-0.466***	-0.336***	-0.513***	-0.728***	-0.499***	-0.642***
	(0.158)	(0.135)	(0.128)	(0.129)	(0.125)	(0.117)	(0.120)	(0.121)	(0.141)	(0.157)	(0.172)
College	-0.272	0.096	0.241*	0.022	-0.119	-0.245*	-0.296**	-0.521***	-0.536***	-0.347**	-0.555***
	(0.166)	(0.143)	(0.138)	(0.139)	(0.137)	(0.128)	(0.129)	(0.129)	(0.152)	(0.169)	(0.182)
Farmer	0.010	-0.143*	0.001	-0.129	-0.013	0.023	0.057	-0.174*	0.004	-0.284**	0.075
	(0.097)	(0.086)	(0.094)	(0.096)	(0.099)	(0.096)	(0.097)	(0.104)	(0.113)	(0.125)	(0.138)
White-collar	-0.064	-0.050	0.024	-0.078	-0.047	0.048	-0.031	0.044	0.072	-0.083	-0.192**
	(0.055)	(0.050)	(0.055)	(0.056)	(0.059)	(0.056)	(0.059)	(0.058)	(0.068)	(0.068)	(0.077)
Professional	0.122*	0.163**	0.095	0.241***	0.161**	0.221***	0.085	0.019	0.142*	0.126	-0.034
	(0.069)	(0.063)	(0.068)	(0.067)	(0.068)	(0.067)	(0.071)	(0.071)	(0.081)	(0.086)	(0.093)
Self-employed	-0.153**	-0.120*	-0.215***	-0.078	0.029	0.018	-0.105	-0.090	0.013	0.140	-0.045
	(0.076)	(0.065)	(0.072)	(0.072)	(0.081)	(0.075)	(0.079)	(0.073)	(0.089)	(0.094)	(0.108)
Civil servant	0.328***	0.213***	0.145**	0.099	0.111	0.227***	0.068	0.239***	0.086	0.235**	0.084
	(0.075)	(0.067)	(0.072)	(0.070)	(0.075)	(0.073)	(0.076)	(0.078)	(0.089)	(0.096)	(0.102)
City	0.033	0.072*	0.013	0.067	0.134***	0.051	0.011	0.001	-0.012	0.017	0.020
	(0.042)	(0.039)	(0.042)	(0.042)	(0.045)	(0.043)	(0.046)	(0.045)	(0.052)	(0.053)	(0.060)
Large City	0.099*	0.105**	0.174***	0.108**	0.090	0.051	-0.007	-0.013	-0.033	-0.030	0.110
	(0.055)	(0.052)	(0.055)	(0.054)	(0.055)	(0.054)	(0.057)	(0.059)	(0.069)	(0.072)	(0.080)
birth	-0.013**	-0.011**	-0.016***	-0.003	0.003	-0.002	-0.000	-0.019***	-0.005	-0.011	-0.003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)
height	-0.000	0.002	0.006**	0.004	0.004	0.010***	0.006**	0.014***	0.009***	0.011***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Partner Variables											
Region (East/ South)	0.149	0.040	-0.031	-0.325*	-0.040	-0.093	-0.215	0.725***	0.051	0.191	0.165
	(0.142)	(0.143)	(0.185)	(0.171)	(0.179)	(0.165)	(0.167)	(0.164)	(0.178)	(0.177)	(0.286)
Foreign origin	0.104	0.031	0.060	0.133	0.166**	0.239***	0.102	0.157*	0.147	0.137	0.226*
	(0.089)	(0.077)	(0.085)	(0.087)	(0.083)	(0.075)	(0.084)	(0.083)	(0.097)	(0.099)	(0.124)
Secondary	0.058	0.003	0.047	0.215*	0.079	0.008	-0.106	0.044	0.261**	0.127	0.303*
	(0.148)	(0.125)	(0.119)	(0.123)	(0.117)	(0.107)	(0.108)	(0.111)	(0.133)	(0.148)	(0.159)
Intermediate	0.005	-0.088	-0.040	0.243*	0.033	0.054	-0.080	0.069	0.214	0.174	0.343**
	(0.156)	(0.132)	(0.127)	(0.129)	(0.125)	(0.117)	(0.120)	(0.123)	(0.143)	(0.157)	(0.169)
College	0.019	0.119	0.002	0.104	0.110	0.002	-0.007	0.171	0.304**	0.290*	0.438**
	(0.165)	(0.141)	(0.138)	(0.139)	(0.137)	(0.128)	(0.132)	(0.129)	(0.152)	(0.168)	(0.180)
Farmer	-0.165*	-0.190**	-0.295***	-0.109	-0.406***	-0.255***	0.015	-0.087	-0.451***	-0.258**	-0.028
	(0.098)	(0.086)	(0.094)	(0.097)	(0.099)	(0.097)	(0.098)	(0.103)	(0.115)	(0.128)	(0.140)
White-Collar	0.022	0.125**	0.003	0.066	0.064	0.032	0.080	-0.080	-0.046	0.111	0.118
	(0.055)	(0.050)	(0.056)	(0.056)	(0.059)	(0.055)	(0.058)	(0.057)	(0.067)	(0.068)	(0.077)
Professional	0.113	0.122*	0.138**	0.145**	0.112	0.197***	0.231***	0.102	0.018	0.129	0.234**
	(0.070)	(0.064)	(0.069)	(0.068)	(0.069)	(0.068)	(0.072)	(0.071)	(0.081)	(0.088)	(0.094)
Self-employed	0.069	-0.017	-0.019	0.007	0.115	0.236***	0.147*	0.014	-0.045	0.134	0.100
	(0.076)	(0.066)	(0.073)	(0.073)	(0.082)	(0.075)	(0.078)	(0.073)	(0.089)	(0.094)	(0.107)
Civil servant	-0.080	-0.033	-0.039	0.072	-0.028	0.044	-0.004	-0.030	-0.154*	0.046	-0.045
	(0.076)	(0.068)	(0.073)	(0.071)	(0.075)	(0.074)	(0.078)	(0.079)	(0.089)	(0.095)	(0.101)
City	0.013	-0.033	0.025	0.053	0.038	0.059	0.053	0.029	0.122**	0.035	-0.021
	(0.042)	(0.039)	(0.042)	(0.042)	(0.045)	(0.044)	(0.046)	(0.045)	(0.052)	(0.053)	(0.060)
Large city	0.008	0.002	-0.091*	0.073	0.129**	0.135**	0.062	0.107*	0.208***	0.109	0.025
	(0.055)	(0.052)	(0.055)	(0.054)	(0.055)	(0.054)	(0.057)	(0.060)	(0.068)	(0.072)	(0.080)
birth	0.003	-0.002	0.004	-0.008	-0.014**	-0.012**	-0.012**	-0.003	-0.002	-0.004	-0.003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)
height	0.006**	0.004	0.004	0.005*	-0.001	0.000	-0.003	0.000	0.002	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Experience	-0.064**	-0.061**	-0.037	-0.011	-0.015	-0.038	-0.011	0.006	-0.042	0.042	0.038
	(0.028)	(0.027)	(0.029)	(0.030)	(0.033)	(0.032)	(0.034)	(0.034)	(0.039)	(0.042)	(0.049)
Sq experience	-0.023	-0.002	0.013	0.024	0.038*	0.013	-0.008	-0.024	-0.017	0.001	-0.042
	(0.018)	(0.017)	(0.018)	(0.018)	(0.020)	(0.019)	(0.021)	(0.021)	(0.024)	(0.025)	(0.028)
Weekly hours	-0.006***	-0.004**	-0.004**	-0.007***	-0.009***	-0.008***	-0.013***	-0.007***	-0.005**	-0.009***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Education	0.027***	0.018**	0.038***	0.050***	0.059***	0.036***	0.051***	0.062***	0.054***	0.040***	0.050***
	(0.009)	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)
Energy and Mining	0.196**	0.147**	0.093	-0.003	-0.121	-0.170**	-0.216**	-0.144*	-0.299***	-0.169*	-0.175
	(0.076)	(0.070)	(0.076)	(0.075)	(0.081)	(0.077)	(0.085)	(0.084)	(0.095)	(0.096)	(0.108)
Engineering	0.183**	0.112	0.011	0.216**	0.050	-0.218**	-0.261**	0.043	0.176	-0.013	-0.025
	(0.089)	(0.081)	(0.087)	(0.092)	(0.097)	(0.098)	(0.101)	(0.093)	(0.108)	(0.113)	(0.125)
Manufacturing	-0.128	-0.062	0.074	-0.091	-0.129	-0.050	-0.107	-0.339***	-0.188	-0.180	-0.097
	(0.090)	(0.085)	(0.095)	(0.092)	(0.101)	(0.106)	(0.109)	(0.111)	(0.130)	(0.126)	(0.131)
Construction	0.079	-0.023	-0.116	-0.053	-0.265***	-0.261***	-0.582***	-0.281***	-0.335***	-0.291***	-0.205*
	(0.083)	(0.078)	(0.089)	(0.089)	(0.098)	(0.094)	(0.097)	(0.096)	(0.108)	(0.111)	(0.123)
Sales	0.045	0.096	0.127*	0.033	-0.052	-0.070	-0.156**	-0.060	-0.073	-0.096	-0.128
	(0.071)	(0.065)	(0.071)	(0.071)	(0.075)	(0.074)	(0.079)	(0.077)	(0.091)	(0.095)	(0.103)
Transport	0.101	0.116	0.200**	0.049	-0.006	-0.224**	-0.485***	-0.026	-0.448***	-0.013	0.202
	(0.097)	(0.086)	(0.094)	(0.092)	(0.100)	(0.105)	(0.111)	(0.107)	(0.129)	(0.138)	(0.185)
Financial	0.218*	0.322***	0.441***	0.362***	0.257*	-0.031	0.158	0.103	-0.027	-0.063	0.011
	(0.121)	(0.115)	(0.126)	(0.127)	(0.134)	(0.119)	(0.129)	(0.130)	(0.140)	(0.146)	(0.151)
Service	0.126*	0.162**	0.251***	0.115	0.003	-0.016	-0.066	0.026	-0.048	-0.155*	-0.249**
	(0.073)	(0.066)	(0.072)	(0.071)	(0.078)	(0.074)	(0.081)	(0.077)	(0.089)	(0.092)	(0.099)
Education	0.109	0.167**	0.137	-0.055	-0.123	-0.033	-0.086	-0.171*	0.043	0.044	0.106
	(0.090)	(0.077)	(0.084)	(0.082)	(0.087)	(0.086)	(0.094)	(0.090)	(0.106)	(0.107)	(0.114)
Health	0.032	0.055	0.090	0.035	0.010	-0.173**	-0.211**	-0.068	-0.075	-0.019	-0.084
	(0.078)	(0.070)	(0.076)	(0.076)	(0.082)	(0.079)	(0.084)	(0.084)	(0.096)	(0.099)	(0.109)
Constant	27.918***	35.149***	29.861***	28.447***	30.017***	35.725***	34.451***	49.500***	22.174***	37.092***	18.870**
Observations	2,136	2,361	2,273	2,148	1,891	1,959	1,825	1,607	1,413	1,239	970
R-squared	0.363	0.368	0.337	0.351	0.369	0.364	0.409	0.432	0.375	0.393	0.391

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.11 – Regression Results Full Responsibility - Net Income (1999-2000)

Variables	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Female	-0.579***	-0.659***	-0.649***	-0.727***	-0.673***	-0.570***	-0.532***	-0.565***	-0.720***	-0.703***
	(0.086)	(0.095)	(0.099)	(0.102)	(0.098)	(0.096)	(0.094)	(0.097)	(0.094)	(0.070)
Foreign origin	0.134	0.108	0.055	0.100	-0.078	-0.124	0.042	-0.304***	-0.641***	-0.338***
	(0.105)	(0.125)	(0.132)	(0.136)	(0.139)	(0.129)	(0.125)	(0.117)	(0.114)	(0.085)
Region (East/ South)	-0.129***	-0.295	-0.658	-0.488*	-0.283	-0.453	-0.087	-0.106	-0.066	-0.267**
	(0.049)	(0.574)	(0.411)	(0.286)	(0.398)	(0.283)	(0.252)	(0.191)	(0.186)	(0.132)
Secondary	-0.391	-0.134	-0.523	0.808	0.275	-0.542	0.115	-0.164	-0.326	-0.242*
	-1.398	-1.559	-1.538	-1.239	-1.170	-1.563	-1.054	(0.321)	(0.330)	(0.140)
Intermediate	-0.390	0.089	-0.430	0.837	0.409	-0.463	0.252	-0.093	-0.183	-0.193
	-1.400	-1.561	-1.541	-1.242	-1.173	-1.564	-1.056	(0.324)	(0.332)	(0.147)
College	-0.324	0.172	-0.340	0.926	0.499	-0.400	0.269	-0.178	-0.285	-0.259*
	-1.401	-1.561	-1.541	-1.243	-1.173	-1.565	-1.057	(0.332)	(0.339)	(0.154)
Farmer	0.146	0.147	0.223*	0.166	0.040	0.039	-0.038	0.048	-0.047	0.104
	(0.111)	(0.111)	(0.121)	(0.127)	(0.111)	(0.109)	(0.108)	(0.107)	(0.107)	(0.081)
White-collar	0.084	0.014	0.014	0.133*	0.072	0.092	0.050	-0.048	0.014	0.013
	(0.059)	(0.066)	(0.068)	(0.070)	(0.066)	(0.065)	(0.065)	(0.063)	(0.061)	(0.048)
Professional	0.168**	-0.097	-0.126	-0.003	-0.047	-0.007	-0.029	-0.023	0.041	0.162***
	(0.073)	(0.079)	(0.085)	(0.087)	(0.080)	(0.079)	(0.077)	(0.076)	(0.069)	(0.059)
Self-employed	0.147*	0.058	0.092	0.018	0.096	0.105	0.106	0.000	-0.088	0.009
	(0.075)	(0.088)	(0.093)	(0.094)	(0.092)	(0.091)	(0.093)	(0.093)	(0.090)	(0.065)
Civil servant	0.042	-0.044	0.161*	0.141	0.097	-0.017	0.148*	0.154*	0.156*	0.163***
	(0.079)	(0.087)	(0.090)	(0.095)	(0.091)	(0.092)	(0.088)	(0.081)	(0.080)	(0.061)
City	0.072	0.032	0.072	0.112**	0.122**	0.048	0.127**	0.095*	0.056	0.063*
	(0.045)	(0.050)	(0.054)	(0.054)	(0.051)	(0.050)	(0.050)	(0.049)	(0.047)	(0.037)
Large City	0.055	0.092	0.094	0.142**	0.115*	0.018	0.114*	0.185***	0.108*	0.138***
	(0.059)	(0.064)	(0.067)	(0.069)	(0.066)	(0.064)	(0.062)	(0.063)	(0.061)	(0.048)
birth	-0.014**	-0.011*	-0.022***	-0.016**	-0.020***	-0.018***	-0.016**	-0.015***	-0.012*	-0.013***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)
height	0.007**	-0.000	0.005	0.003	0.001	0.004	0.003	0.009***	0.006*	0.003
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)
Partner Variables										
Region (East / South)		0.262	0.709*	0.428	0.196	0.354	0.049	0.166	-0.068	0.180
		(0.574)	(0.412)	(0.288)	(0.398)	(0.284)	(0.253)	(0.193)	(0.187)	(0.132)
Foreign origin	-0.018	0.087	-0.076	-0.001	0.049	0.125	0.037	-0.060	-0.080	-0.060
	(0.111)	(0.130)	(0.139)	(0.143)	(0.147)	(0.135)	(0.129)	(0.124)	(0.124)	(0.087)
Secondary	-0.859	-0.795	-1.003	0.303	-0.580	-1.282	-0.725	-0.636*	-0.579*	-0.123
	-1.445	-1.610	-1.598	-1.247	-1.178	-1.640	-1.059	(0.336)	(0.336)	(0.140)
Intermediate	-0.920	-0.704	-1.095	0.322	-0.646	-1.246	-0.763	-0.825**	-0.715**	-0.118
	-1.447	-1.613	-1.601	-1.250	-1.181	-1.642	-1.062	(0.339)	(0.338)	(0.147)
College	-0.774	-0.773	-1.246	0.312	-0.660	-1.336	-0.742	-0.675*	-0.788**	-0.238
	-1.448	-1.614	-1.602	-1.251	-1.182	-1.643	-1.063	(0.345)	(0.344)	(0.153)
Farmer	0.115	-0.031	-0.012	0.121	0.086	0.046	-0.047	-0.171	-0.304***	-0.097
	(0.110)	(0.111)	(0.123)	(0.131)	(0.112)	(0.112)	(0.108)	(0.107)	(0.107)	(0.081)
White-Collor	0.061	0.003	0.019	0.112	0.092	0.003	0.117*	0.093	0.122**	0.044
	(0.059)	(0.066)	(0.068)	(0.069)	(0.066)	(0.065)	(0.064)	(0.063)	(0.061)	(0.048)
Professional	0.156**	0.030	0.136	0.111	0.051	-0.053	0.102	0.153**	0.203***	0.082
	(0.072)	(0.080)	(0.086)	(0.086)	(0.080)	(0.078)	(0.076)	(0.076)	(0.069)	(0.060)
Self-employed	0.165**	0.006	0.166*	0.151*	0.166*	-0.081	0.071	0.097	0.013	0.003
	(0.073)	(0.087)	(0.092)	(0.092)	(0.092)	(0.091)	(0.092)	(0.092)	(0.089)	(0.066)
Civil Servant	0.058	0.010	0.204**	0.047	0.174*	0.074	0.177**	-0.082	0.048	0.130**
	(0.078)	(0.086)	(0.090)	(0.095)	(0.091)	(0.092)	(0.088)	(0.082)	(0.080)	(0.061)
City	0.068	-0.025	0.024	0.006	0.041	0.017	-0.017	-0.070	0.033	0.052
	(0.045)	(0.050)	(0.053)	(0.054)	(0.052)	(0.050)	(0.050)	(0.050)	(0.047)	(0.037)
Large City	0.102*	0.036	0.070	0.029	0.061	0.058	0.037	-0.050	0.025	0.021
	(0.059)	(0.064)	(0.067)	(0.069)	(0.066)	(0.064)	(0.062)	(0.064)	(0.061)	(0.048)
birth	0.006	0.001	0.011*	0.004	0.013**	0.005	0.002	-0.003	-0.001	-0.002
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)
height	0.001	0.001	0.004	0.004	-0.001	0.001	0.002	0.003	0.009**	0.005*
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)
Experience	-0.051*	-0.038	-0.039	-0.059*	-0.003	-0.030	-0.058*	-0.010	-0.057*	-0.030
	(0.027)	(0.031)	(0.033)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.033)	(0.024)
Sq experience	-0.018	-0.032	-0.009	0.011	0.005	-0.014	-0.001	0.015	-0.013	-0.019
	(0.018)	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)	(0.023)	(0.021)	(0.020)	(0.015)
weekly hours	0.000	-0.004	-0.006**	0.003	0.005**	0.001	0.000	-0.004*	0.004**	0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
education	-0.004	0.001	0.013	0.027**	0.021*	0.036***	0.016	0.026**	0.051***	0.038***
	(0.011)	(0.012)	(0.013)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.008)
Energy and Minig	-0.150*	-0.047	-0.086	-0.196**	-0.143	-0.163*	-0.079	-0.106	-0.115	-0.001
	(0.077)	(0.084)	(0.089)	(0.093)	(0.087)	(0.088)	(0.088)	(0.087)	(0.086)	(0.065)
Engineering	-0.164*	-0.178*	-0.110	-0.101	-0.104	-0.168	-0.176*	0.044	-0.140	0.006
	(0.091)	(0.104)	(0.110)	(0.105)	(0.101)	(0.107)	(0.105)	(0.106)	(0.097)	(0.076)
Manufacturing	-0.019	0.023	-0.037	-0.156	-0.159	-0.064	-0.284***	0.123	-0.005	-0.063
	(0.094)	(0.109)	(0.114)	(0.116)	(0.114)	(0.112)	(0.109)	(0.109)	(0.109)	(0.080)
Construction	-0.022	-0.051	-0.105	-0.244**	-0.038	-0.158	-0.027	-0.077	-0.027	-0.003
	(0.092)	(0.100)	(0.105)	(0.103)	(0.100)	(0.097)	(0.103)	(0.107)	(0.097)	(0.074)
Sales	-0.155**	-0.145*	-0.211**	-0.170*	-0.185**	-0.185**	-0.240***	-0.063	-0.199**	-0.134**
	(0.079)	(0.086)	(0.091)	(0.091)	(0.087)	(0.088)	(0.087)	(0.086)	(0.085)	(0.063)
Transport	-0.168	-0.235**	-0.128	-0.211*	-0.370***	-0.197*	-0.054	-0.160	-0.286**	-0.094
	(0.105)	(0.114)	(0.126)	(0.125)	(0.124)	(0.119)	(0.120)	(0.121)	(0.117)	(0.084)
Financial	0.061	0.101	0.050	-0.015	0.080	0.241*	0.247*	0.350***	0.039	0.066
	(0.118)	(0.129)	(0.134)	(0.133)	(0.130)	(0.135)	(0.134)	(0.127)	(0.128)	(0.112)
Service	-0.060	-0.010	-0.043	-0.302***	-0.028	-0.008	0.015	0.184**	0.088	-0.002
	(0.092)	(0.095)	(0.098)	(0.097)	(0.095)	(0.096)	(0.089)	(0.087)	(0.082)	(0.062)
Education	0.052	0.071	-0.144	-0.155	0.088	0.122	0.109	0.077	-0.140	0.012
	(0.097)	(0.102)	(0.104)	(0.102)	(0.100)	(0.103)	(0.105)	(0.101)	(0.098)	(0.073)
Health	-0.128	-0.172*	-0.160	-0.161	-0.138	-0.110	-0.234***	-0.017	-0.116	0.054
	(0.090)	(0.093)	(0.099)	(0.102)	(0.095)	(0.092)	(0.089)	(0.088)	(0.088)	(0.068)
Income	-0.229***	-0.256***	-0.180***	-0.239***	-0.313***	-0.337***	-0.372***	-0.230***	-0.321***	-0.344***
	(0.039)	(0.037)	(0.039)	(0.038)	(0.039)	(0.037)	(0.036)	(0.034)	(0.037)	(0.027)
Constant	27.483***	31.997***	32.922***	31.999***	26.791***	39.660***	39.645***	46.545***	35.399***	42.735***
	-5.563	-6.412	-6.898	-6.945	-6.646	-6.906	-6.739	-6.775	-6.274	-4.797
Observations	1,182	1,162	1,13	1,124	1,124	1,092	1,076	1,202	1,21	2,146
R-squared	0.433	0.414	0.409	0.396	0.429	0.447	0.452	0.439	0.455	0.443

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.12 – Regression Results Full Responsibility - Net Income (2001-2011)

Variables	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Female	-0.799***	-0.720***	-0.621***	-0.722***	-0.613***	-0.489***	-0.579***	-0.574***	-0.584***	-0.585***	-0.505***
	(0.071)	(0.065)	(0.066)	(0.070)	(0.074)	(0.070)	(0.074)	(0.076)	(0.087)	(0.090)	(0.099)
Foreign origin	-0.265***	-0.218***	-0.136*	-0.137*	-0.186***	-0.262***	-0.225***	-0.318***	-0.322***	-0.448***	-0.274**
	(0.077)	(0.067)	(0.071)	(0.075)	(0.072)	(0.065)	(0.072)	(0.073)	(0.084)	(0.086)	(0.109)
Region (East/ South)	-0.192	-0.145	-0.064	0.156	-0.026	0.058	0.137	-0.582***	-0.231	-0.304*	-0.177
	(0.125)	(0.127)	(0.159)	(0.150)	(0.157)	(0.145)	(0.148)	(0.148)	(0.158)	(0.155)	(0.252)
Secondary	-0.303**	-0.019	-0.008	0.028	-0.000	-0.107	-0.324***	-0.420***	-0.578***	-0.375***	-0.548***
	(0.132)	(0.113)	(0.103)	(0.109)	(0.102)	(0.094)	(0.095)	(0.098)	(0.116)	(0.129)	(0.140)
Intermediate	-0.334**	-0.120	-0.107	-0.105	-0.197*	-0.356***	-0.324***	-0.433***	-0.657***	-0.408***	-0.524***
	(0.140)	(0.119)	(0.111)	(0.114)	(0.110)	(0.104)	(0.107)	(0.109)	(0.126)	(0.138)	(0.152)
College	-0.263*	0.084	0.071	-0.020	-0.081	-0.157	-0.259**	-0.435***	-0.469***	-0.264*	-0.438***
	(0.147)	(0.127)	(0.119)	(0.123)	(0.121)	(0.112)	(0.115)	(0.115)	(0.136)	(0.148)	(0.161)
Farmer	0.025	-0.067	0.018	-0.077	0.003	-0.007	0.101	-0.018	0.018	-0.303***	0.136
	(0.086)	(0.076)	(0.081)	(0.085)	(0.087)	(0.084)	(0.086)	(0.093)	(0.101)	(0.110)	(0.121)
White-collar	-0.062	-0.019	0.025	-0.071	-0.036	0.047	-0.016	0.025	0.065	-0.049	-0.168**
	(0.049)	(0.044)	(0.047)	(0.049)	(0.052)	(0.049)	(0.052)	(0.052)	(0.060)	(0.059)	(0.068)
Professional	0.133**	0.163***	0.114*	0.245***	0.185***	0.239***	0.164**	0.075	0.176**	0.195**	0.039
	(0.061)	(0.056)	(0.059)	(0.059)	(0.060)	(0.059)	(0.064)	(0.064)	(0.072)	(0.076)	(0.082)
Self-employed	-0.082	-0.081	-0.142**	-0.061	0.107	0.046	-0.037	-0.012	0.082	0.187**	-0.035
	(0.067)	(0.058)	(0.062)	(0.064)	(0.071)	(0.066)	(0.070)	(0.066)	(0.079)	(0.083)	(0.095)
Civil servant	0.288***	0.197***	0.104*	0.112*	0.152**	0.206***	0.099	0.245***	0.092	0.248***	0.138
	(0.067)	(0.060)	(0.062)	(0.062)	(0.066)	(0.064)	(0.068)	(0.070)	(0.079)	(0.084)	(0.090)
City	0.016	0.043	0.010	0.049	0.103***	0.033	0.004	-0.005	0.018	0.030	0.014
	(0.037)	(0.035)	(0.036)	(0.037)	(0.039)	(0.038)	(0.041)	(0.040)	(0.046)	(0.046)	(0.053)
Large City	0.071	0.072	0.124***	0.086*	0.066	0.042	-0.018	0.023	0.011	-0.004	0.100
	(0.049)	(0.046)	(0.048)	(0.048)	(0.048)	(0.048)	(0.051)	(0.053)	(0.061)	(0.063)	(0.070)
birth	-0.010**	-0.009**	-0.013***	-0.001	0.003	-0.002	-0.001	-0.016***	-0.005	-0.009	-0.002
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.008)
height	-0.000	0.003	0.006***	0.004	0.003	0.009***	0.004	0.012***	0.007**	0.009***	0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Partner Variables											
Region (East / South)	0.113	0.040	-0.040	-0.258*	-0.035	-0.070	-0.174	0.522***	0.036	0.117	0.084
	(0.126)	(0.127)	(0.159)	(0.151)	(0.158)	(0.145)	(0.149)	(0.149)	(0.159)	(0.156)	(0.252)
Foreign origin	0.058	0.022	0.064	0.128*	0.149**	0.185***	0.111	0.152**	0.115	0.060	0.209*
	(0.079)	(0.069)	(0.074)	(0.077)	(0.074)	(0.066)	(0.075)	(0.075)	(0.086)	(0.087)	(0.110)
Secondary	-0.042	-0.005	-0.081	0.151	0.049	-0.017	-0.118	0.031	0.161	0.120	0.229
	(0.132)	(0.111)	(0.103)	(0.108)	(0.103)	(0.094)	(0.097)	(0.100)	(0.119)	(0.130)	(0.140)
Intermediate	-0.076	-0.066	-0.155	0.151	-0.001	0.008	-0.109	0.020	0.064	0.122	0.227
	(0.139)	(0.117)	(0.110)	(0.114)	(0.110)	(0.103)	(0.108)	(0.111)	(0.128)	(0.137)	(0.150)
College	-0.047	0.115	-0.101	0.078	0.069	-0.023	-0.018	0.126	0.188	0.270*	0.346**
	(0.147)	(0.125)	(0.119)	(0.123)	(0.121)	(0.113)	(0.117)	(0.116)	(0.136)	(0.147)	(0.159)
Farmer	-0.139	-0.126*	-0.208**	-0.081	-0.287***	-0.230***	0.050	0.027	-0.318***	-0.281**	0.072
	(0.087)	(0.076)	(0.081)	(0.085)	(0.087)	(0.085)	(0.087)	(0.092)	(0.102)	(0.112)	(0.123)
White-Collor	0.011	0.107**	0.013	0.043	0.043	0.038	0.064	-0.072	-0.047	0.061	0.087
	(0.049)	(0.045)	(0.048)	(0.050)	(0.052)	(0.049)	(0.052)	(0.051)	(0.060)	(0.059)	(0.068)
Professional	0.127**	0.108*	0.122**	0.144**	0.118*	0.209***	0.233***	0.112*	0.064	0.150*	0.195**
	(0.062)	(0.057)	(0.059)	(0.060)	(0.061)	(0.059)	(0.064)	(0.064)	(0.072)	(0.077)	(0.083)
Self-employed	0.060	-0.010	-0.010	-0.008	0.140*	0.202***	0.157**	0.049	0.002	0.151*	0.114
	(0.068)	(0.059)	(0.063)	(0.065)	(0.072)	(0.066)	(0.069)	(0.065)	(0.079)	(0.082)	(0.094)
Civil Servant	-0.031	-0.014	-0.039	0.083	0.030	0.063	-0.001	0.010	-0.084	0.089	0.019
	(0.067)	(0.060)	(0.063)	(0.063)	(0.066)	(0.065)	(0.069)	(0.071)	(0.079)	(0.083)	(0.089)
City	-0.016	-0.041	0.009	0.028	0.019	0.052	0.038	0.012	0.113**	0.035	-0.025
	(0.037)	(0.035)	(0.036)	(0.037)	(0.040)	(0.038)	(0.041)	(0.040)	(0.047)	(0.047)	(0.053)
Large City	0.002	0.001	-0.063	0.067	0.093*	0.126***	0.047	0.119**	0.211***	0.107*	0.057
	(0.049)	(0.046)	(0.048)	(0.048)	(0.049)	(0.048)	(0.051)	(0.054)	(0.061)	(0.063)	(0.071)
birth	0.003	-0.001	0.003	-0.005	-0.010*	-0.010**	-0.009*	-0.003	-0.001	-0.004	0.001
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.008)
height	0.005*	0.003	0.005**	0.004*	-0.000	0.002	-0.003	0.001	0.004	0.005*	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Experience	-0.048*	-0.031	-0.011	0.002	0.006	-0.023	0.004	0.023	-0.018	0.065*	0.051
	(0.025)	(0.024)	(0.025)	(0.026)	(0.029)	(0.028)	(0.030)	(0.031)	(0.036)	(0.038)	(0.044)
Sq experience	-0.028*	-0.008	0.007	0.021	0.023	0.005	-0.006	-0.030	-0.032	-0.020	-0.038
	(0.016)	(0.015)	(0.015)	(0.016)	(0.018)	(0.017)	(0.018)	(0.019)	(0.021)	(0.022)	(0.025)
weekly hours	0.001	0.002	0.003*	-0.001	-0.000	-0.002	-0.004**	0.001	0.003	0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
education	0.029***	0.028***	0.041***	0.053***	0.063***	0.043***	0.054***	0.068***	0.065***	0.056***	0.069***
	(0.008)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)	(0.012)
Energy and Minig	0.145**	0.115*	0.059	-0.037	-0.128*	-0.197***	-0.247***	-0.183**	-0.344***	-0.199**	-0.206**
	(0.068)	(0.062)	(0.066)	(0.066)	(0.072)	(0.068)	(0.076)	(0.075)	(0.085)	(0.084)	(0.095)
Engineering	0.115	0.045	-0.044	0.160**	-0.010	-0.313***	-0.285***	-0.015	0.049	-0.057	-0.051
	(0.079)	(0.072)	(0.075)	(0.081)	(0.085)	(0.087)	(0.090)	(0.084)	(0.097)	(0.099)	(0.110)
Manufacturing	-0.215***	-0.109	-0.011	-0.137*	-0.224**	-0.142	-0.178*	-0.356***	-0.298**	-0.254**	-0.186
	(0.080)	(0.076)	(0.082)	(0.081)	(0.089)	(0.094)	(0.098)	(0.100)	(0.117)	(0.111)	(0.116)
Construction	0.042	-0.012	-0.118	-0.022	-0.228***	-0.221***	-0.511***	-0.266***	-0.301***	-0.261***	-0.243**
	(0.074)	(0.070)	(0.077)	(0.079)	(0.087)	(0.083)	(0.086)	(0.086)	(0.096)	(0.097)	(0.109)
Sales	-0.056	0.021	0.007	-0.059	-0.090	-0.141**	-0.220***	-0.097	-0.120	-0.130	-0.130
	(0.064)	(0.059)	(0.062)	(0.064)	(0.066)	(0.066)	(0.071)	(0.069)	(0.082)	(0.083)	(0.091)
Transport	0.023	0.066	0.104	0.037	-0.027	-0.244***	-0.440***	-0.066	-0.456***	-0.048	0.149
	(0.086)	(0.076)	(0.081)	(0.082)	(0.088)	(0.092)	(0.099)	(0.096)	(0.115)	(0.121)	(0.163)
Financial	0.125	0.260**	0.367***	0.300***	0.213*	-0.104	0.122	0.104	0.034	0.029	0.058
	(0.107)	(0.102)	(0.108)	(0.112)	(0.118)	(0.105)	(0.115)	(0.116)	(0.125)	(0.128)	(0.133)
Service	0.072	0.119**	0.193***	0.094	-0.036	-0.075	-0.085	0.021	-0.055	-0.159**	-0.198**
	(0.065)	(0.059)	(0.063)	(0.063)	(0.069)	(0.066)	(0.072)	(0.070)	(0.080)	(0.081)	(0.088)
Education	0.092	0.164**	0.123*	-0.036	-0.096	-0.064	-0.062	-0.200**	0.047	0.049	0.087
	(0.079)	(0.069)	(0.072)	(0.073)	(0.077)	(0.075)	(0.083)	(0.081)	(0.094)	(0.094)	(0.101)
Health	-0.003	0.055	0.016	-0.026	-0.027	-0.182***	-0.286***	-0.096	-0.100	-0.051	-0.036
	(0.069)	(0.062)	(0.066)	(0.068)	(0.072)	(0.070)	(0.075)	(0.075)	(0.086)	(0.087)	(0.096)
Income	-0.273***	-0.265***	-0.315***	-0.265***	-0.328***	-0.283***	-0.284***	-0.294***	-0.325***	-0.393***	-0.333***
	(0.027)	(0.026)	(0.026)	(0.027)	(0.028)	(0.027)	(0.029)	(0.032)	(0.034)	(0.038)	(0.041)
Constant	26.376***	30.593***	29.515***	22.869***	26.059***	33.556***	31.455***	46.839***	22.506***	36.387***	11.315
	-4.898	-4.568	-4.880	-5.209	-5.899	-5.676	-6.024	-6.006	-6.977	-7.053	(8.315)
Observations	2,136	2,361	2,273	2,148	1,891	1,959	1,825	1,607	1,413	1,239	970
R-squared	0.436	0.428	0.418	0.416	0.441	0.437	0.472	0.484	0.440	0.470	0.458

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4 Jobs, Crime, and Vote - a short-run analysis of the German refugee crisis.

4.1 Introduction

Immigration has become one of the more contentious issues in the public discourse on policies related to labor markets, crime, trade, and the political economy. The debate has intensified in light of the recent inflow of refugees to Europe. The main goal of this paper is to use this sharp and unexpected rise in the number of migrants coming to Germany in 2014/2015 as a natural experiment in order to evaluate its short-run effect on unemployment, crime, and voting behavior. In this, we build on numerous studies that have investigated the impact of migration and immigration, often with a focus on labor market outcomes. Arguably, a consensus has not been reached. For example, Card (2001) and Dustmann et al. (2013) find very moderate or insignificant effects of immigrant inflows on natives' wages and employment prospects. Borjas (2003a) and Aydemir and Borjas (2007), on the other hand, show substantial negative effects of immigration on natives' labor market outcomes. These conflicting findings can be explained by differences in the model assumptions, in particular the degree to which natives and immigrants are substitutes (Borjas et al., 2012; Card, 2012a). Moreover, the frequently applied spatial correlations approach, which divides data into cells based on geography and skill levels, is prone to endogeneity issues not least because immigrants are likely to differentially sort into regions that offer them the best employment prospects. As a result, a range of natural experiments such as the Mariel boatlift (Card, 1990; Borjas, 2015) the relocation of Algerian repatriats to France (Hunt, 1992), spatial dispersal policies (Edin et al., 2003; Gould et al., 2004; Damm, 2009a), or border openings after the fall of the Berlin wall (Dustmann et al., 2016) have been exploited to get a better sense of the effect of immigration on labor market outcomes.

In the context of the effect of immigration on crime, there has been slightly less research, studies by Butcher and Piehl (1998) for the US, Bell et al. (2013a) for the UK, and Bianchi et al. (2012a) for Italy being notable exceptions. These studies have found no effects of immigration on violent crime and, at best, moderate effects on property crimes. However, **?** find a positive association between crime and immigration in their longitudinal analysis of Germany, the country we study in this article. Finally, voting behavior and attitudes towards immigrants are an interesting research frontier (Dustmann and Preston, 2007, among others). Mayda (2006) shows that individual skills are strong predictors of attitudes towards immigration. Card (2012a) develops his concept of compositional amenities and shows that concerns about the social effects of immigration often outweigh concerns about its economic effects.

Our study serves two purposes. First, we contribute to the literature by exploiting a natural experiment that was created by the allocation mechanisms in Germany during the refugee crisis in 2014/2015. We show that within states, migrants were allocated to counties based on reasons unrelated to local labor market conditions or crime levels. In particular, we provide evidence that neither incomes nor the skill compositions of natives differ substantially between high and low migration counties. Housing vacancies are also not significant determinants of refugee allocations, although it remains conceivable that the availability of estates that can house a large amount of refugees all in one place, e.g. abandoned barracks, is a predictor. More importantly, counties that experience small refugee inflows and those with large inflows appear to follow identical time trends in terms of unemployment, crime and voting patterns. This allows us to obtain credibly causal effects on less stringent identification assumptions.

Second, our study provides a first evaluation of the short-run consequences of the refugee crisis in Germany, an event that features prominently in the public discourse. We exploit a plausibly exogenous source of variation in migrant inflows to determine the effect of these inflows on unemployment, crime, and voting behavior. Using a difference-in-differences framework with continuous treatment, we find no evidence for displacement of native workers by refugees. However, our findings suggest difficulties in integrating refugees into the German labor markets. These difficulties are likely to worsen as more and more migrants become eligible to legally enter the labor market. Our findings are consistent with earlier studies for Germany, such as Pischke and Velling (1997) and D'Amuri et al. (2010), and stand in contrast with Glitz's (2012a) study who exploits the exogenous inflow of ethnic Germans from the Soviet Union. His research design is probably the most similar to ours, although substantial differences remain, not least because the inflows in the 1990s were smaller on a per-year basis and the time horizon Glitz (2012a) was able to evaluate was longer. Our study also suggests that - with the obvious exception of violations to right-of-residence and asylum laws - there is no association between the number of refugees and the number of street crimes in Germany. However, we do find a statistically significant relationship between bigger reception centers and drug crimes and fare-dodging, as well as the number of non-German suspects in relation with theses crimes. This might partly be driven by higher alertness of police in these counties. In general, crime only increased marginally more in counties which received larger refugee inflows. Finally, there is no indication that (micro-)exposure to refugees either increases or decreases propensities to vote for anti-immigrant parties or affects voter turnout, although our analysis of voting behavior is limited to four states.

The remainder of this paper is structured as follows. In the next section, we provide background information on the refugee crisis and how the German institutional setting dealt with the inflow of hundreds of thousands of migrants in 2014 and 2015. Section 4.3 introduces our data, in particular the newly collected administrative records that document the distribution of migrants across counties. Section 4.4 describes the empirical setup and the assumptions our identification strategy is built on. We present our results in Section 4.5, discuss them in Section 4.6 where we also provide a few robustness checks and conclude in Section 4.7.

4.2 Background

In 2011, the year the Syrian civil war erupted, only 50,000 asylum applications were filed in Germany (BAMF, 2016). From 2014 on, more and more people embarked on their journey towards Europe. Most of them took the so-called "Balkan route", crossing the Mediterranean, often on make-shift boats, from Turkey into Greece. From there they traveled onwards through countries of former Yugoslavia towards Western Europe. In theory, asylum applications in the European Union (EU) are governed by the Dublin Regulation which shifts the responsibility of administering an asylum request to the first EU member state a migrant sets foot on. In practice, few refugees had any intention of staying in Greece (or Hungary), but tried to travel on to, among other countries, Austria, Germany, or Sweden, as these countries promised better living conditions, more generous welfare benefits and better job perspectives. By late summer 2015, amid images of refugees being stuck in trains and camps in Hungary, the German government is essence abandoned the Dublin Regulation and allowed all refugees who had passed through other EU countries to file for asylum in Germany.

At this point, the inflow changed from a steady increase to a large jump in daily arrival rates, with thousands of new immigrants seeking asylum at the German border every day. Figure 4.1 attests to this immigration shock. In 2015 alone 1,091,894 refugees were registered at the German border (BMI, 2016). The inflows were only curtailed when a deal was forged between the EU and Turkey in early 2016, in which Turkey committed to crack down on people smugglers in return for €6 billion in aid earmarked for humanitarian support of refugees who have fled to Turkey. The deal effectively closed the Balkan route. For example, Figure 4.1 shows that in April 2016 only 15,941 refugees were registered in Germany.

The aforementioned number of 1,091,894 refugees coming to Germany in 2015 insinuates that on the federal level exact data on the number of arrivals exist. Unfortunately this is only partly true. While every asylum seeker who is picked up by the German border police undergoes a quick check, the actual registration takes place in separate reception centers. Between quick check and registration, numerous ways to unilaterally exit the asylum procedure exist. For example, little is known about the number of refugees who continued their journeys to other countries and left with asylum claims pending. To the best of our knowledge, we are the first to collect detailed data on the allocation of registered asylum seekers to German counties, by obtaining administrative data of the states and counties. These data unfortunately include no information on refugee characteristics. To this end, the best information to date come from the Federal Office for Migration and Refugee's asylum (BAMF) statistics. However, these data are based only on asylum claims that have been fully processed. For example, in 2015 when about 1.1 million migrants entered the country, only 476,649 asylum applications were processed which included backlog from 2014 (BAMF, 2016). Based on this information about 69.2 percent of applicants were male, about 31 percent were younger than 18 years old and only 6.6 percent were older than 45. About 35.9 percent of asylum seekers were from Syria. About 20 percent were from Albania and Kosovo and only about 0.1 percent of applications from citizens of these two countries were approved. In fact both countries were officially declared "safe countries of origin" in 2015, thus substantially speeding up asylum procedures and reducing the inflow from these countries. For example, Albania only accounted for 2 percent of processed asylum applications in April 2016.

The German authorities had a top-to-bottom system in place to deal with refugee inflows. Newly arrived refugees were supposed to be received by the federal police at their points of entry, often at train stations close to the Austrian border.[1] After a quick check by the federal police, most refugees were placed in short-term facilities for a couple of days, before being transferred to a federal state with free capacities.[2] These allocations were to follow a quota, the so called "Koenigssteiner Schluessel". This quota is determined by a state's tax revenues and population, thus ensuring that the costs related to housing and processing of asylum claims are evenly distributed. Each state runs so called reception centers (Erstaufnahmeeinrichtungen, EAEs). EAEs tend to have large-scale housing facilities. Only there, more detailed information was gathered from the prospective asylum claimers and entered into the EASY System. Applicants are obliged to stay in their assigned reception center for a period of up to six months during the processing of their application. Violations of these residential obligations lower the chances of being granted asylum. After this period, or - more often - if the

[1] Even in this first step, not all refugees could be processed, the BAMF estimates that up to 290,000 persons have not been registered at all.

[2] The standard procedure provides that new arrivals are transferred to the closest reception center, where their personal information is entered into EASY, a federal database. The EASY system subsequently allocates new arrivals to one of Germany's 16 states for further processing of their asylum claims.

BAMF decides that the application cannot be processed in a timely manner, migrants are redistributed within the same state to subordinate counties ("Landkreise").[3] Due to efficiency gains and a lack of available space, county authorities tend to provide communal accommodations rather than allowing asylum seekers to seek individual apartments.

An ideal natural experiment would feature an entirely random allocation of refugees to counties, with some counties receiving large inflows and other counties receiving small inflows regardless of their characteristics. The actual quasi-experiment provided by the refugee crisis at the very least resembles this ideal case and creates exogenous variation due to housing shortages and the sheer necessity to relocate refugees from the German border: refugees were usually transported from border regions in Bavaria to other states by trains and buses on a daily basis. Deviations of the actual distribution quotas - both the state-quotas and within-state quotas - were inevitable and mainly arose from housing capacity shortages and inseparable groups. Due to the overwhelming volume of inflows, state authorities usually simply allocated migrants to counties that had some kind of accommodation facilities to spare, for example because they happened to be home to recently abandoned military barracks, or sports halls that could be transformed into collective accommodations, or recently closed hotels, etc.. The availability of suitable housing might not be entirely independently distributed across counties but as we will show in Section 4.4, the resulting inflows were by and large uncorrelated with economic and social county characteristics. Moreover, allocation decisions were made by state authorities, and within states counties are subject to very similar labor market conditions and crime fighting strategies.

Several pull and push factors incentivize asylum seekers to stay in their designated county. For one, asylum seekers are provided with goods and social services at their accommodations or nearby reception centers. Second, refugees are legally obliged to reside in their assigned accommodations until a decision has been made on their asylum claim. Violations against this "residence obligation" negatively affect the probability of having one's asylum claim approved. The average processing time for asylum applications is about half a year and is highly dependent on an asylum seeker's country of origin and the types of documents he/she can provide.[4] However, an asylum procedure is not

[3]Each state has the authority to distribute asylum seekers to subordinate counties according to its own legislation ("Rechtsverordnungen"). Usually refugees were supposed to be allocated to counties commensurate with their population. But all states include a clause in their legislation that allowes for deviations under extraordinary circumstances. Section 4.3 will show that invoking theses clauses and deviating from the scheduled distribution schemes quickly became the norm rather than the exception.

[4]According to the federal police only about 20-30 percent of refugees entering the country were in possession of a passport (GdP, 2015). In general, Syrian asylum seekers, whose applications have a high probability of being approved, and asylum seekers from the Balkans, whose application have little chance of being approved, are processed with priority.

usually initiated immediately upon arrival. Instead an initial "interview" appointment has to be scheduled which usually involves waiting times of several months. In other words, asylum seekers are tied to a county for very substantial time. In the meantime, they are legally prohibited from working, and only once an application is fully approved can they freely enter the labor market. Ultimately, the scramble to somehow place refugees in what was often make-shift housing resulted in large differences in the number of refugees hosted by counties that in other dimensions followed strikingly similar time trends. It is exactly this source of exogenous variation we exploit in this study.

4.3 Data

For our analysis, we combine several data sources, the most important of which are administrative records by the 16 German states on the allocation of refugees to the 402 subordinate German counties. These records are usually maintained by the states' internal affairs ministries, or in some instances by a state-run agency that supervises the allocation of refugees to the counties. While the German freedom of information act ("Informationsfreiheitsgesetz") only applies to federal agencies, most states have similar laws in place and the competent authorities in all 16 states provided records on the assignment of refugees to counties in the years 2014 and 2015 for all 16 states.[5] By and large, all states abided to the same reporting standards, making those data comparable across states.

Aside from coordinating the transfer of migrants to counties and communities, states also run the above mentioned large-scale reception centers (EAEs). We obtained detailed information on the location and capacities of these EAEs directly from the competent authorities of 8 states. 4 other states pointed us to their website where the same information could be retrieved. For the three city states - Berlin, Hamburg und Bremen - which are equally state and county, there is no clear distinction between state-run EAEs and county-level accommodations.

Table 4.1 shows the number of migrants that were allocated to the counties by the states according to our data in 2014-15. It is notable that these numbers are more or less in line with the shares of refugees that were supposed to be received by states by virtue of the federal quota. For example, Germany's most populous state, Northrhine-Westphalia (NRW) was due to receive 21.21 percent of refugees entering the country, according to the federal key. In our data about 24.4 percent of refugees were allocated

[5]One state provided data only for 2015. The 2014 data were imputed based on the absolute number of refugees allocated to this state and assuming an distribution across counties that is identical to that of 2015.

to NRW counties. Note that the allocated percentage does not necessarily have to be identical to the federal quota since some of the federally allocated migrants might be housed in state-run EAEs rather than allocated to the counties. This is especially true for Bavaria through which most immigrants who took the Balkan route entered the country; similarly, Baden-Wuertemberg and Hesse have large (state-administered) EAE capacities and correspondingly somewhat lower county allocations.

As mentioned in Section 4.2, federal data on the number of registered asylum seekers are scarce and often incomplete.[6] In light of this, our data is the best estimate of county-allocations of refugees to date and probably draws a more accurate picture of refugee allocations than the federal data base could. Despite the issues with federal data in general and the EASY system in particular, it is comforting that the data provided by the states are roughly consistent with the federal allocation key.

Based on the administrative records provided by the states, we calculate the number of allocated refugees per 100,000 inhabitants for each county. Figure 4.2 illustrates that there is quite a bit of variation across counties, even within states. Crosses indicate the presence of an EAE in a county. In some instances, counties in which a particularly large state-run EAE has been set up were allocated fewer migrants. Other than that there is no obvious, discernible pattern in the allocation of refugees within states, although some states certainly achieve a more even allocation across counties than others. Yet, a fair amount of variation remains (the average refugee allocation is 1,088 per 100,000 inhabitants with a standard deviation of 378). This is vital for our study which will exploit county differences in refugee allocations to isolate the effect of additional refugee inflows on labor market, crime, and election outcomes.

Put differently, our identification strategy (more details are provided in Section 4.4), requires that refugee allocations are independent of any time trends in the residuals ("common time trend assumption"). In order to investigate whether this identifying assumption is met, we split our sample into high and low migration counties. High migration counties are defined as counties which host an EAE with a capacity of at least 200 beds or have been allocated more than 1,260 refugees per 100,000 inhabitants, which puts them roughly into the 25th percentile in terms of this measure. This rule achieves a 50:50 split into high and low migration counties.

Unemployment data are provided by the Federal Labor Office on a quarterly basis from 1/2005 to 1/2016. Figure 4.4 plots the unemployment rates separately for the general population and for non-German workers. Three things stand out. First, unemployment rates for non-Germans are substantially higher than for the "native" population. The non-German unemployment rate also warrants a closer look as newly

[6] The EASY system has also been widely criticized for containing duplicates and migrants that continued their journey to other countries.

arrived job seekers might be better substitutes for existing foreign workers, thus exacerbating an existing lack of integration into the labor market for this particular group. And indeed, there is a notable increase in non-German unemployment in the first quarter of 2016. However, at first glance, this increase seems only slightly more pronounced in counties with high refugee inflows than in those with low inflows. Second, no such up-tick is obvious for overall unemployment. This is a first indication that overall unemployment has not been much affected by refugee inflows. Figure 4.3b supports this notion by plotting changes in unemployment rates between the first quarter of 2013 and the first quarter of 2016 for all counties. A comparison with Figure 4.3a, indicates that changes in unemployment are for the most part uncorrelated with migrant inflows. Finally, Figure 4.4 shows that unemployment *levels* tend to be slightly higher in counties that receive a large migrant influx. But more importantly, there is no difference in unemployment *trends* in the pre-treatment period. Both low migration counties and high migration counties experience the same seasonality patterns and have experienced the same decline in unemployment throughout the 2000s and 2010s.

We also obtained data on criminal activity and criminal suspects which are released by the Federal Criminal Police Office on an annual basis. Figure 4.5a plots trends in reported crimes separately for high and low migration counties. It should be noted that not all cases are solved and that minor infractions and petty crimes are not recorded. The graph reveals a large increase in the number of criminal offenses per 100,000 inhabitants in 2014 and 2015 when the refugee crisis was in full swing. At first blush, this might suggest that the refugee crisis was accompanied by a crime epidemic. However, much of this increase can be explained by an increase in violations related to asylum and right-of-residence laws. By definition, any refugee who enters Germany on the land route will be in violation of the Dublin Regulation, although in practice few of these violations were actually recorded. What is more, asylum seekers whose applications were rejected and who remain in the country illegally will inflate these numbers. Once we adjust the time series by discarding these types of offenses, the up-tick in crime disappears, in fact the crime rate seems to have not budged at all.[7]

More important for our identification strategy is that the number of committed crimes follows very similar pre-crisis time trends in low and high migration counties. This also holds true when we look at different categories of crime. For example, the number of street crimes (bag-snatching, bike thefts,...) declined to the same extent during pre-treatment period in counties that were to experience large and small migrant inflows in 2014/2015 (see Figure 4.5b). Likewise, the number of drug-related

[7]It should be noted that we could only adjust the time series for 2014 and 2015 since transgressions of asylum and right-of-residence laws were not reported on a per-county-basis prior to 2014. However, in 2013 these offenses only accounted for 1.85 percent of all offenses nationwide, so that the amount of (downward) bias that is induced by this adjustment should be negligible.

offenses appears to have remained flat in both types of counties. Figure 4.3c which shows the change in aggregate crime (adjusted for asylum and right-of-residence law transgressions) lends further support to the notion that differential migrant influxes appear to be unrelated with changes in crime rates.

The refugee crisis has also had profound impacts on the political landscape in Germany. Therefore, we collected data on vote shares and polls for Germany's largest anti-immigration party, "Alternative fuer Deutschland" (AfD). The AfD party was founded in early 2013. At the time, its main platform was opposition to the Euro and the Euro zone bailouts. Figure 4.6 shows bi-weekly AfD party polls. The first vertical line indicates the 2013 federal election in which the AfD party received 4.7 percent of votes, thus failing to clear the constitutional 5 percent threshold to receive any seats in the federal parliament. Over time, the AfD party's focus arguably turned from Euro-scepticism towards immigration. The second dashed vertical line is placed at 5 September 2015. On this day the German chancellery allowed the entry, by train, of hundreds of refugees who had been detained and were stuck in Hungary. This event is widely seen as the beginning of the refugee crisis with migrant inflows intensifying in the following weeks and months. It also seems to have been associated with an increase in approval for the AfD party which ever since has consolidated its position. In fact, national polls understate the electoral success the AfD party has had. It received between 15.1 and 24.3 percent of votes cast in early 2016 state election. Polls are not taken at the county level, so that we cannot track and compare AfD party support by "treatment intensity", i.e. across high and low migration counties. Instead, we will evaluate whether - within states that held state-elections in 2016 - the electoral success of the AfD party has increased differentially in counties that experienced large migrant inflows relative to the party's performance in the 2013 federal election. Consequently, only a subset of counties (those in states that held state elections in 2016) can be evaluated. If Figure 4.3d is any indication, then inflows of refugees are no important predictors of the AfD party's electoral success which is a result that will be confirmed by our regression analysis in Section 4.5.

Finally, the 2011 Zensus provides us with a variety of county characteristics. Each county's population, per capita GDP (in €), a county's age structure, the share of the population with migration background, the share of the population with a college or vocational degree and the number of housing vacancies (per 1,000 county inhabitants) were sampled.[8] We will use these characteristics to explore to what extent the allocation of refugees to counties constitutes an exogeneous shock. Table 4.2 indicates

[8]Only the population estimates are updated annually, all other county covariates are only available as of 2011, i.e. lagged and without time variation. This issue will receive more attention in Section 4.4.

that high migration and low migration counties differ only marginally along observable dimensions. For example, 74.2 percent of the population in high migration counties have a college or vocational degree which is similar to the 73.0 percent in low migration counties. The only notable difference is that per capita GDP in 2011 was higher in counties that were to experience large migrant inflows. This should not be surprising since the federal allocation quota arranges for larger contingents to be allocated to economically stronger states. We will see in Section 4.4 that once state specific characteristics are accounted for, these differences by and large disappear.

4.4 Methodology

All five data sources - administrative state records on refugee allocations and EAE capacities, unemployment rates as provided by the Federal Labor Office, the Federal Criminal Police Office's crime data, official federal and state election outcomes, and Zensus 2011 results – are subsequently matched with one another at the county level. For each outcome, we have at least one observation per county prior to the refugee crisis in 2013 and one observation pertaining to either 2015 or 2016. As the number of refugees assigned to a certain county potentially depends on the share of asylum seekers the county received before, we include data for 2014 refugee distribution. That is, we pool the 2014 and 2015 figures, in order to create a comprehensive measure of refugee inflows.[9] This gives rise to a specification of the following form:

$$y_{ct} = \delta_c + \gamma D_{2015/16} + \pi_1 D_{2015/16} \cdot ref_c + \pi_2 D_{2015/16} \cdot EAE_c + \epsilon_{ct} \tag{4.1}$$

where y_{ct} is a measure of our three outcomes of interest - unemployment rate, crime rate, and AfD party vote share - in county c at time t. δ_c denotes a full set of county dummies, $D_{2015/16}$ is an indicator for the post-treatment period. Our coefficients of interest are π_1 and π_2, which are related to interactions of the post-treatment dummy, $D_{2015/16}$, and the number of refugees that were allocated to a county between 1 January 2014 and 31 December 2015, ref_c, and the EAE capacities, EAE_c , that were put into operation over the same time period. π_1 thus measures to what extent counties which experienced a larger influx of refugees have experienced larger increases in unemployment, crime, and voting behavior.

Our empirical setup differs from a classic difference-in-differences setup in two ways. First, all units of observations receive the treatment (i.e. inflows of migrants) but the

[9] As a robustness check, we later also treat the 2015 and 2014 inflow separately and evaluate how changes in inflows between these two years are associated with changes in outcomes.

intensity of this treatment differs across counties.[10] Second, we only observe outcomes at two points in time. Once in the pre-treatment period and once in the post-treatment period respectively.[11] That is, unemployment rates are evaluated in the first quarter of 2013 and the first quarter of 2016; we evaluate changes in crime from 2013 to 2015[12]; and changes in the AfD vote share from the federal election in September 2013 to the state elections in early 2016 for the counties that are located in states that held a state election. With just two observations per county, equation 4.1 is equivalent to a first-differencing specification of the following form:

$$\triangle\, y_c = \beta_0 + \pi_1 ref_c + \pi_2 EAE_c(+\theta X_c) + \eta_c \tag{4.2}$$

where $\triangle y_c$ measures the change in outcome y_c. Note that π_1 and π_2 in equation 4.2 by definition must be equal to the same coefficients in equation 4.1 and thus still measure the impact of differential migrant inflows on our outcomes of interest. This specification has the advantage of allowing us to explicitly include time-invariant county-level covariates: X_c denotes county characteristics such as GDP per capita, average age of the population, share of the population with a migration background and a college or vocational degree, and the number of housing vacancies (per 1,000 inhabitants), all of which are per Zensus 2011.

One major challenge to the validity of our estimates of the relationship between refugee inflows on the one hand, and unemployment, crime, and voting behavior changes on the other hand, is that high and low migration counties might differ along dimensions that predict differential refugee allocations. For example, if refugees were primarily allocated to counties in economic decline, our model would pick up spurious, positive correlation between unemployment and refugee inflows. In an ideal empirical setup, on the other hand, refugees would be randomly assigned to counties, thus creating differential exogenous shocks. The institutional setup in Germany provided for neither a negatively selective nor random assignment of refugees to counties. After all, allocation quotas require economically stronger states to absorb larger inflows. Nonetheless, Table 4.3 shows that after controlling for state fixed effects, only one of our observable county characteristics is an individually significant (at the 5 percent level) predictor of the number of refugees allocated to a county and the size of this effect is moderate at best, indicating that a one standard-deviation increase in the

[10]In terms of this feature, our study resembles, among others, the prominent work of Acemoglu et al. (2004) who investigate the effect of differential mobilization rates across US states during World War II on female labor supply.

[11]In this respect, the empirical setup of our study resembles Card and Krueger's (1994) seminal study on the effect of the minimum wage increase in New Jersey.

[12]Note that 2016 county level crime data will only become available over the course of 2017.

share of population with a migration background predicts an allocation of 54 fewer refugees per 100,000. In other words, within-state refugee inflows into a county are mostly uncorrelated with observable county characteristics. It should be stressed that our empirical setup does not (even) require this very strong assumption of random refugee inflows to hold. Equation 4.2 will yield an unbiased estimate of the differential effect of migrant inflows as long as the residuals in low migration and high migration counties are subject to the same time trends. Figures 4.4 and 4.5 support this common time trend assumption. Still, the fact that few of our observable characteristics are significant predictors of refugee inflows experienced by the counties lends additional support to this identifying assumption.

It is also notable that in Table 4.3 housing vacancies are no significant predictors of refugee allocations. However, anecdotal evidence suggests that the presence of a single large property that allows for the accommodation of many refugees in one facility, e.g. former army ("Bundeswehr") barracks, might be a strong predictor of immigrant inflows. Unfortunately, there seems to exist no conclusive list of abandoned barracks, so that we cannot entirely dismiss the notion that the presence of such a property leads to non-random allocations of refugees across counties.[13] Even if having hosted a military base in, say, the 1980s was associated with larger refugee inflows today, this would only threaten the validity of our estimates if barracks had been closed selectively and closures had differential effects on our outcomes of interest. In light of the fact that with the end of the cold war barracks all over the country became obsolete and were closed, such a narrative seems unlikely.

4.5 Results

4.5.1 Refugees and Unemployment

Our regression analysis estimates the *differential* effect of migrant inflows, i.e. whether counties with high migration inflow experience larger increases in unemployment, crime, and voter turnout. Our descriptive statistics in Table 4.2 suggest that this is hardly the case. In both low and high migration counties unemployment actually decreased slightly.

The results in Table 4.4 confirm this finding. If anything, local unemployment rates and migrant inflows are negatively related although this relationship is neither

[13]There is a surprisingly detailed list of several hundred abandoned Bundeswehr properties on Wikipedia. According to this list, virtually all West-German counties are home to a former army, navy, or air-force base. However, the Bundeswehr could not confirm the accuracy nor the completeness of said list. Nor is there any information on which facilities are suitable for accommodation.

statistically nor economically significant once covariate controls are included. Nor have the presence or the capacities of a reception center any influence on the overall unemployment rate. The same is true for the unemployment rate of youths aged 15 to 25 (see columns (3) and (4) of Table 4.4). The vast majority of working-age migrants are between 16 and 25 years old and they will often look for apprenticeships or entry level positions which may put them into competition with young native workers (BAMF, 2016). Even so, our estimates suggest that there is little in the way of a displacement effect.

Another group of potential substitutes are non-German workers and pre-crisis immigrants, many of whom may possess similar skill sets. And indeed, larger inflows of migrants are associated with increases in the unemployment rate for workers who are not German citizens. Column (8) of Table 4.4 suggests that a one standard deviation increase in migrant inflows is associated with a 1.2 percentage point increase in the unemployment rate for non-Germans. Given the 2013 average unemployment rate for this group, this estimate translates into about a 7.6 percent increase. There are two plausible explanations for this striking increase in non-German unemployment. For one, refugees may have displaced some non-German workers and pushed them into unemployment. This may very well have happened through the shadow economy as refugees can only legally enter the workforce once their asylym claim has been approved.[14].

A second explanation is that recently arrived refugees themselves start to show up in the unemployment statistics. This would indicate difficulties of the German labor market to immediately absorb this influx of additional job seekers. There is some evidence supporting this causal chain. On the county level, no information on the country of origin of job seekers is available; yet such information is compiled on the federal level. Figure 4.7 plots these data. On the left-hand side y-axis we measure the overall number of non-German job seekers. Between the third quarter of 2015 - which is also the time when substantial numbers of refugees should have started to receive work permits - and the first quarter of 2016 about 150,000 additional non-German job seekers registered with the Federal Employment Agency. During the same time period the number of job seekers from the eight main crisis countries (Syria, Iraq, Afghanistan, Iran, Pakistan, Nigeria, Eritrea, and Somalia) increased by roughly the same number, indicating that the absolute increase in non-German unemployment is mostly driven by recent refugees seeking work. Note that the data underlying Figure 4.7 use a different definition of unemployment and include workers who are part of government-sponsored programs,

[14]There is an alternative route for refugees to obtain a work permit. However, this route is subject to a complex approval process which among other things involves a priority check of whether there is no other job seeker from an EU country who is potentially being displaced.

e.g. to enhance their skills. The county-level data underlying Table 4.4, on the other hand, would not count job seekers who are taking part in active labor market policy programs as unemployed. The simultaneous increase in non-German unemployment and unemployment of citizens from the main crisis countries is striking. It indicates that our regression estimate does not reflect displacement effects. Instead, our result might best be interpreted as evidence for difficulties of migrant workers to quickly integrate into the German labor market. These difficulties appear to be quite substantial. For example in all of 2015 only 137,136 people were granted asylum and thus received a work permit (2014 total was 31,025). In early 2016 processing speed picked up and 92,577 asylum claims were approved in the first three months of 2016. The magnitude of the increase in unemployment indicates that many of those who have obtained a work permit by way of an approved asylum claim struggled to find employment. This problem appears to be particularly grave considering that not everybody who was granted asylum intends to become part of the labor force. For example, the BAMF estimates that about two thirds of Syrian women are neither in employment nor looking for work (Worbs and Bund, 2016). Similarly, many minors who were granted asylum are more likely to attend school than show up in the unemployment statistics. Hence, the marked increase in non-native unemployment which parallels the increase in the number of immigrants who were granted asylum (and thus became eligible to work) indicates substantial difficulties of the German labor market to absorb this labor supply shock, at least in the short-run. Not surprisingly these difficulties tend to be more pronounced in counties that received larger refugee inflows.

4.5.2 Refugees and Crime

Table 4.5 shows the effects of refugee inflows on crime rates. Panel A looks at the aggregate crime rate (per 100,000) and is adjusted for the natural increase in offenses related to immigration and asylum laws. Even after immigration offenses are excluded from the crime statistics, the number of refugees allocated to a county is significantly and positively associated with increases in crime (see columns (1) and (2) of Panel A in Table 4.5). A one-standard deviation increase in migrant inflow is associated with about 95 additional crimes per 100,000. Given a mean of 6,417 crimes per 100,000, this translates into roughly a 1.5 percent increase.[15] Since 2013, the official crime statistics distinguish between German and non-German crime suspects. While refugees only make up a fraction of the non-German population, increases in the number of crime

[15] We also used a log-level specification as a sensitivity check for crime results. Table A3 in the appendix confirms our findings on overall crime rates.

cases with non-German main suspects would support the hypothesis of immigration induced increases in crime. We indeed find a positive association between larger migrant inflows and the number of non-German suspects. Yet again, these are very moderate in size. Columns (5)'s and (6)'s coefficients suggest that a 1 standard deviation increase in refugee allocations increase the number of cases involving a non-German suspect by about 54 (mean is 625). Hence, these increases - while not negligible - show no sign of exploding crime rates. All results are robust to the inclusion of covariates, largely supporting our identification strategy.

Aggregate crime statistics also contain all types of offenses ranging from very serious crimes, such as assault, to smaller transgressions such as fare-dodging. We thus separately evaluate different types of crime. For example, street crimes account for a little less than a quarter of crimes in Germany and include all offenses that take place in the public sphere such as handbag-snatching, damages to motor vehicles, theft from kiosks and show windows, bike-nicking, breaking of vending machines, and (attempted) robberies of money vans. Our regression analysis detects no differential increases in these crimes in counties that host more refugees. Nor is there any indication that the number of non-German suspects for street crimes is associated with migrant inflows. Our results for drug-related crimes are displayed in Panel C of Table 4.5. We find substantial and statistically significant effects for EAE capacities. 200 extra beds per 100,000 inhabitants, which is roughly the average county capacity, are associated with an increase of about 4.4 drug offense in a county. The mean number of drug offenses (per 100,000) in our sample is 317.74, so this estimate suggests that the presence of an average-sized EAE is associated with an increase in drug-related crime of about 1.4 percent. Interestingly enough, our analysis of suspects in cases involving a drug offense suggests that the presence of a reception center is associated with significant increases in both the number of German and non-German suspects. An increase in 200 reception center beds increases the number of German suspects by 2.90 (mean: 231.04) and the number of non-German suspects by 1.1 (mean: 47.00). In absolute terms, much of the increase in drug-related crimes is therefore driven by "native criminals", although in relative terms the increase in non-German suspects for drug-related crimes is more pronounced. Of course, we have no way of knowing how many of the non-German suspects are recent refugees.[16] In other words: while our result indicate that counties with larger EAEs have seen larger increases in drug-related crimes, we cannot conclude with certainty that refugees were the offenders in those crimes. Our results do, however, provide suggestive evidence that EAEs might be potential "hotspots" for drug offenses, although it is also conceivable that the authorities have devoted more

[16] In contrast to the unemployment data, publicly available federal crime statistics do not report suspects by nationality which may have been informative in this respect.

resources to policing these areas so that crimes are more likely to be recorded in the first place. Panel D evaluates fare-dodging offenses. Anecdotal evidence suggests that these have become more common, and indeed we find a positive correlation between EAE capacities and the number of offenses and the number of non-German suspects. Yet again, these are relatively small effects. A 200 bed increase in the number of EAE spots increases the number of faredodging offenses by about 2 percent and the number of non-German suspects by about 8 percent.[17]

4.5.3 Refugees and Voting Behavior

An analysis of voting behavior is complicated by the fact that elections do not take place every year or even quarterly. The last federal election in Germany took place in September 2013, the next federal election will take place in 2017. Three elections for state parliaments took place in early 2016 in Baden-Wuertemberg, Rhineland Palatinate, and Saxony-Anhalt; municipal elections (at the county level) took place around the same time in Hesse. Our main outcome of interest is the vote share for the anti-immigrant AfD party. We also analyze election turnout and evaluate the support for the incumbent party which appoints a state's prime minister. The AfD party did not exist at the time of the last state elections in the aforementioned states. Therefore, we rely on the 2013 federal election outcomes as a proxy for the AfD party's baseline support prior to the refugee crisis. Furthermore, the AfD party changed its political focus between 2013 and 2016 from opposition to the Euro bailouts towards migration issues, so our results have to be interpreted with some caution.

Table 4.6 shows no statistically significant effect of refugee inflows or EAE capacities on the electoral success of the AfD party. Of course, this is not to say that the refugee crisis has not helped the AfD party in achieving larger electoral success. Figure 4.6 strongly suggests that gains in approval are driven by concerns about immigration. Our results, however, indicate that these gains were no more pronounced in counties that actually received larger inflows than in those with smaller inflows of migrants. In other words, direct (micro-) exposure to migrants neither increases nor decreases a county's constituents' propensity to cast their votes for the AfD party. Figure 4.3d is a case in point. The AfD party was particularly successful in the eastern state of Saxony-Anhalt. This success was, however, not accompanied by large refugee inflows to Saxony-Anhalt. Rather, both far-left and far-right parties tend to traditionally fare better in East-Germany than in the West. Our results hold up, regardless of whether

[17] Note that we also evaluated other crime types and found similar results for the number of property damages, violent crimes in general and assaults in particular. These results are available from the authors.

the municipal elections in Hesse - which may be deemed less important than state elections and saw a turnout of just 48 percent - are included in the sample. Controls for county level characteristics also do not change the results.[18]

We also find no indication that more voters took to the ballots in counties with larger refugee inflows. In fact, turnout is by and large uncorrelated with refugee inflows. However, our results indicate that the incumbent party suffered heavier losses in counties with larger immigrant inflows than in those with small inflows. In fact, column (7) of Table 4.6 indicates that a one standard deviation increase in refugee inflows is associated with a loss of 4.5 percentage points in the share of votes cast for the incumbent party. Our results stand in contrast to a study by Steinmayr (2016) who found that Austrian districts with large refugee presence were less likely to vote for anti-immigration parties.[19] Barone et al. (2016), on the other hand, find that larger immigrant inflows are associated with better election outcomes for center-right parties.[20] It should, however, be stressed that our results only rely on a small subset of all German states and counties. It will, therefore, be of interest to analyze future elections.

4.6 Discussion and Sensitivity

Our paper provides a first evaluation of the refugee inflow to Germany in 2014-2015. It is necessarily an analysis of short-run effects. As such, there is no guarantee that trends we have uncovered in this study will hold in the long-run. Even over the course of conducting this study, new events in Germany and abroad have occurred that might shape debates and policies. Nonetheless, our analysis of short-term effects provides interesting insights that might contribute to an evidence-based debate on the economic and social effects of large migrant inflows in general and the consequences of the recent wave of refugees in particular. In a nutshell, our analysis suggests: migrants did not displace natives; crime only marginally increased with larger refugee inflows; and differential exposure to refugees is largely uncorrelated with support for anti-immigration parties. At the same time, our results indicate that the labor supply shock induced by the refugee crisis has not yet been fully absorbed by the German labor market. The identifying assumption under which these results are most credible is that trends

[18]Voting districts do not always align with county borders. We are grateful to the statistical offices of the states to aggregate the election results to the county level for us.

[19]Of course, both the party platforms and the setup of the refugee allocation mechanism are different in Austria.

[20]Again, this comparison is slightly flawed since Barone et al. (2016) evaluate a pre-crisis time period, have much more detailed data on immigration (for over 8,000 districts), and evaluate voting shares for more established anti-immigration parties in Italy

in employment, crime, and voting behavior would have been the same in high migration counties as in low migration counties in the absence of refugee inflows. We have provided evidence that suggests that this is a fair assumption to make. Placebo tests provide another piece of evidence for the validity of our identification strategy. For that purpose, we move the time window of analysis into a time-period that was unaffected by the refugee crisis. Specifically, we re-estimate equation 4.2 for the years 2011 and 2013 (rather than 2013 and 2015/16) and attribute the refugee inflows that actually took place in 2014/15 to the year 2012/13.[21]

Our results for this analysis are displayed in Table 4.7. We cannot detect any effect of our placebo refugee inflows on the overall unemployment rate, youth unemployment, or unemployment of non-Germans. This is comforting for two reasons. First, it lends additional credibility to our zero effect finding for overall and youth unemployment rates. Second, our finding that larger inflows of refugees are associated with increases in non-German unemployment does not appear to be driven by the fact that counties with large inflows were on a different unemployment trajectory prior to the start of the refugee crisis.

Admittedly, our placebo results for crime are somewhat less convincing. It appears as if counties that were to absorb larger migrant inflows had been on a slight downward trajectory in terms of overall crime. Fortunately, there is little evidence for such a downward trajectory for drug offenses where the coefficients on our placebo refugee variable are negative but insignificant. Recall that for drug offenses, we found a positive association with EAE capacities, and here our placebo test is comforting in that it suggests that the crime rate trajectory was similar across counties which did and those that did not become EAE sites.[22] By and large, our placebo tests lend additional credibility to our results for unemployment and indicate that we might slightly underestimate the effect of refugee inflows on crime although the amount of bias is relatively small. There is no evidence for large undetected effects on crime or even a "crime epidemic" due to refugee inflows.

In Appendix Table A2 we repeat our analysis for unemployment and crime using pre-treatment data dating back to 2005. We include year and county fixed effects, but due to a lack of variation over time cannot include covariates. For unemployment, the point estimates are very similar to those in our main specification. For example in the regression for non-German unemployment, the coefficient is 0.0031 when in Table 4.4 containing our preferred specification it was 0.0032. Similarly, we continue to find a

[21] Obviously, we cannot conduct this exercise for our voting behavior outcome as the AfD party was only founded in February 2013.

[22] A placebo analysis separately for German and Non-German suspects is unfortunately not feasible; on the county level this distinction was made for the first time in 2013. Neither can we analyze fare-dodging behavior as this type offense was not reported on a county-level basis prior to 2013.

significant effect of EAE capacity on the number of drug crimes and no relationship between refugee allocations and street crimes. For aggregate crime, the coefficient is similar but ceases to be statistically significant. By and large, including several pre-treatment periods has little impact on our estimates.[23]

From a policy point of view, the results of our analysis of short-run labor market effects is a mixed bag. On the one hand, there is little indication for a displacement of native workers by immigrants. On the other hand, refugees do not appear to be readily absorbed into the labor market, at least in the short time period that we are able to observe. It is conceivable that the relative inflexibility of the German labor market (relative to the US or UK) might be an obstacle to a quick labor market integration of immigrant workers. Brücker et al. (2014) show that this might result in large unemployment effects. Our results lend some support to calls for additional labor market flexibility (Bofinger et al., 2015). Another reason for the slow integration of migrants into the labor market might be skill mismatches. Woessmann (2015) estimates that about two thirds of recent arrivals have "not been sufficiently educated to participate in a modern society". This rather awe-inspiring assessment suggests that Figure 4.7 shows by no means the end of the story, i.e. further increases in non-German unemployment are to expected if more and more unskilled workers enter the labor force. At the very least our results suggest that the unemployment rates of crisis country nationals should be closely tracked, data on the qualifications of migrants need to be collected, and - especially if the aforementioned estimates about the skill level distribution turn out to be correct - training and re-qualification efforts will have to be stepped up.

There is also little indication for large increases in crime, at least within the time period that is covered by our data. Crime rates are generally flat in particular for street crimes, although we find a small uptick in drug related offenses and fare-dodging in counties that host receptions centers. Two other types of crimes that have received substantial public interest could, unfortunately, not be fully evaluated in this study. First, anecdotal evidence suggests that crimes against refugees, and arson attacks against accommodation facilities in particular, are on the rise. Crime statistics do not separately report arson attacks specifically aimed at refugee accommodations. The number of arson cases in Germany actually declined between 2013 and 2015 from 20,009 to 19,251 reported incidents.[24] County level data is only available for 2015 (and not 2013). We ran a cross-sectional analysis and did not find evidence that arsons are more frequent in counties that received a larger inflow of migrants or have larger EAE capacities.

In the same vein, the 2015/16 new year's eve events in Cologne during which many

[23] Our estimates are also robust to including our treatment variables separately (see Appendix Table A2)

[24] This includes not just actual arson attacks but also the criminal act of creating fire hazards.

women were assaulted by men of Arab or North African appearance, have led to a widespread perception that sex crimes committed by refugees have become a major issue. Unfortunately, we can shed little light on this debate. The Cologne events will only show up in the 2016 crime data and county level data of these types of offenses have only recently been collected so that, again, we can merely conduct a cross-sectional analysis for 2015. Such an analysis fails to find any statistically significant association between the number of refugees that were allocated to a county and the number of sex crimes in said county.[25] As a final robustness check, we use the difference in refugees and EAE capacities from 2014 to 2015 as explanatory variables (see Table 4.8). This boils down to a comparison of county-level changes in outcomes with changes in refugee inflows between 2014 and 2015. The results are generally consistent with our previous findings. In particular, we still observe an increase in non-German unemployment of similar magnitude.

4.7 Conclusion

The inflow of more than a million refugees to Germany in 2014/15 continues to influence the German economy and society. It also represents a unique natural experiment that allows for an investigation of labor market, crime, and voting behavior effects of migrant inflows. We analyze the short-term impacts of this largely unanticipated shock and make three related contributions.

First, our results are highly relevant for policy makers. To the best of our knowledge, this is the first study to evaluate the labor market effects of a key event that has shaped public discourse throughout the world. We show that a significant labor supply shock of low skilled prime-age workers has not had much of a displacement effect on native workers. At the same time, our analysis raises some concerns about the ability of the German labor market to absorb this supply shock. This paper is, of course, an analysis of short-term effects. At this early stage in the post-inflow period, our results suggest that policy makers need to devote more resources to labor market integration of migrants. Together with other measures designed to ease the entry of refugee job seekers into the job market, this should help to avoid further increases in non-native unemployment and the associated adverse economic and societal consequences. At the very least, the job seeking experience of eligible refugees needs to be monitored more closely. While we cannot entirely rule out a displacement of native workers in the long-run, there is little sign of this as of now.

[25] The results for both sex crimes and arson attacks are available from the authors upon request.

With respect to crime rates, we find at best muted increases in criminal activity. Again, these are short-run effects and continued monitoring of the situation is warranted. In particular, the release of quarterly or even monthly (rather than annual) crime data might help in this respect. Moreover, we neither want to discount nor emphasize the degree to which attempted and actual terrorist attacks have been affected by refugee inflows and have taken a strain on police and counter-terrorism resources. But, given the data available for non-terrorism related crime and given the time period for which said data were available, there is little evidence for large increases in crime in the immediate aftermath of refugee inflows. Lastly, while the rise of the anti-immigration AfD party is undeniable, there is little indication that counties that experience larger migrant inflows largely vote for said party. However, we find some evidence for a negative association between support for the governing party and the number of refugees assigned to a county.

A second contribution of this paper is the collection of unique county-level data on migrant inflows. The data underlying our analysis accompany this paper and should be tremendously useful to other researchers. For instance, an obvious avenue for future research is the analysis of labor supply shocks on native wages. The data collected for our study will also be helpful in learning more about immigrant sorting as eventually migrants are no longer required to reside in the counties that they were initially allocated to.

Finally, our study deploys a research design that is based on a credible natural experiment. As such it advances the literature on labor market impacts of immigration, sheds additional light on the link between immigration and crime, and provides insights on the effect of immigration on voting behavior. Of course, the natural experiment created by refugee inflows to Germany differs markedly from other natural experiments that have been evaluated in the past. The sheer size of the refugee inflows in such a short time period is unprecedented and has created unique problems in terms of the provision of adequate accommodation, schooling, and social services. Moreover, the presumed skill composition as well as language and cultural barriers might adversely affect both economic and social integration. That is in contrast to, say, the relocation of ethnic Germans after the fall of the Soviet Union who shared the language and culture of their host country (Glitz, 2012a) or the relocation of Cuban migrants to Miami in the wake of the Mariel boatlift (Card, 1990). Some of the migration in our natural experiment might also be of transient nature as a fair number of asylum seekers may return to their home countries eventually. Since the subject matter of this study are at times divisive issues, we want to stress that our results should be interpreted as short-term effects. But, the short-term effect identified in this paper have shown some persistence: the native unemployment rate has continued to drop in 2016/17 while the absolute number of non-German job-seekers has continued to increase[26]; a recent report by the Federal Ministry of the Interior shows that on aggregate crime rates have been largely flat in 2016 (PKS, 2017), albeit with increases in violent crimes and drug crimes which is consistent with our findings.

While prima facie, our results offer useful indications for long-term effects, they are certainly not the last word on this important issue. Given the contentiousness of the debate, we encourage more research on this topic. The natural experiment presented by the refugee inflows provides a useful setting to evaluate their effects and design evidence-based policies. We hope that this paper provides a conclusive and convincing analysis of the short-term effects of the refugee crisis in Germany and can serve as a starting point for future analyses of what is likely to remain a major economic and social issue for years to come.

Tables and Figures

Table 4.1 – Refugee Allocations to States' Subordinate Counties and EAE Capacities

		County Allocations		
	Federal Quota	total	percent	EAE capacities
Baden-Wuertemberg	12.8%	105,680	11.5%	26,400
Bavaria	15.5%	106,763	11.6%	22,377
Berlin	5.1%	67,228	7.3%	n/a
Brandenburg	3.1%	30,930	3.4%	5,092
Bremen	1.0%	12,507	1.4%	n/a
Hamburg	2.5%	28,937	3.1%	n/a
Hesse	7.4%	57,575	6.3%	22,047
Mecklenburg Western Pomerania	2.0%	22,614	2.5%	989
Lower Saxony	9.3%	84,475	9.2%	5,028
Northrhine-Westphalia (NRW)	21.2%	224,589	24.4%	16,245
Rhineland Palatinate	4.8%	34,999	3.8%	10,622
Saarland	1.2%	13,265	1,4%	1,300
Saxony	5.1%	41,423	4.5%	16,845
Saxony-Anhalt	2.8%	27,736	3.0%	6,259
Schleswig-Holstein	3.4%	36,500	4.0%	15,667
Thuringia	2.7%	24,657	2.7%	6,951
Total	100.0%	919,878	100.0%	148,414

Table relates federal quota ("Koenigssteiner Schluessel") of migrants who are supposed to be allocated to the states to the number of refugees forwarded by states to their subordinate counties and the capacities that exist to house refugees in state-run reception centers (EAEs). Berlin, Bremen, and Hamburg are city states and have no subordinate counties, hence no distinction between refugees that are housed by counties and those in state-run facilities is possible. In the data the EAE capacities are coded as zero for all three city states.

[26]Unfortunately, the Federal Labor Office ceased to publish unemployment rates for non-Germans in 2017.

Table 4.2 – Summary Statistics: Table of Means

	(1)	(2)	(3)	(4)	(5)	(6)
		2013 (Pre-Treatment)			2015/16 (Post-Treatment)	
	All	High Migration	Low Migration	All	High Migration	Low Migration
Outcomes						
Refugees per 100,000	0	0	0	1,088	1,196	979.3
EAE Capacity per 100,000	0	0	0	220.1	433.4	6.802
Unemploment Rate (Total)	6.922	7.754	6.091	6.180	6.880	5.481
Youth Unemployment Rate	6.140	6.860	5.420	5.506	6.106	4.905
Unemployment Rate German	5.481	6.206	4.756	3.489	3.984	2.995
Unemployment Rate Non-German	15.44	17.33	13.55	18.30	20.99	15.61
Crimes per 100,000 (unadjusted)	6,417	6,953	5,882	6,792	7,314	6,270
Crimes per 100,000 (adjusted)	6,417	6,953	5,882	6,217	6,777	5,656
German Suspects per 100,000	2,127	2,236	2,019	1,960	2,055	1,864
Non-German Suspects per 100,000	624.8	662.2	587.4	650.7	672.2	629.1
Percentage AfD	5.07	4.94	5.152	14.98	14.98	14.97
County Characteristics as of Zensus 2011						
Population	200,308	230,191	170,425	201,984	235,838	168,129
Average Age	43.58	43.76	43.41	43.58	43.76	43.41
Share w. Migration Background	16.74	16.07	17.40	16.74	16.07	17.40
GDP per Capita	30,993	31,215	30,772	30,993	31,215	30,772
College or Vocational Education	0.736	0.742	0.730	0.736	0.742	0.730
Housing Vacancies	4.709	4.918	4.500	4.709	4.918	4.500
Observations	402	201	201	402	201	201

Table of means. Column (1) shows the means in outcomes as of 2013, prior to the refugee crisis. Column (2) and (3) display the means for the same outcomes for counties that were to experience a large inflow of migrants and counties that were to experience a small inflow of migrations. High migration counties are counties with refugee inflows of at least 1,260 people per 100,000 in population or which opened a reception center with at least 200 beds. Low migration counties are counties that meet neither condition. Columns (4) through (6) display outcomes of interest in the post-treatment period. That is, the first quarter of 2016 for employment outcomes, the 2015 annual aggregate for crime outcomes, and the early 2016 election outcomes. Information on county characteristics are calculated from the Zensus 2011. They therefore do not vary over time with the exception of population estimates which are updated annually. Note that crimes rates could not be adjusted for the pre-treatment period as immigration-related offenses were not listed on a county basis in 2013.

Table 4.3 – Potential Determinants of Refugee Inflows

		Regression					
	Mean	(1)	(2)	(3)	(4)	(5)	(6)
Housing Vacancies	4.709	8.6516					-4.0070
	(21.698)	(11.3353)					(15.2865)
GDP per Capita	30.993		-0.0000				0.0033*
	(13.265)		(0.0013)				(0.0018)
Average Age	43.58			23.6200*			22.5817
	(1.708)			(13.2445)			(17.7066)
Share Migration Background	16.74				-5.6940**		-8.9686**
	(9.498)				(2.6694)		(3.6465)
Percentage Degree	73.63					756.9438	171.5604
	(5.599)					(659.4232)	(744.1184)
Observations		402	402	402	402	402	402
R-squared		0.2648	0.2637	0.2697	0.2723	0.2662	0.2830
State-FE		Yes	Yes	Yes	Yes	Yes	Yes

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses. Each column is a separate county-level regression of the number of refugees (per 100,000) allocated to a county on county characteristics (as per Zensus 2011). All estimates are adjusted for state fixed effects, each county receives the same weight. Housing vacancies are the number of empty living spaces per 1,000 inhabitants, GDP per Capita is measured in €1000, average age is the average age per county, % migration background is the percentage of county population with a migration background (includes ethnic Germans who emigrated from the former Soviet Union after the fall of the iron curtain), % College/Degree is the share of the population with a college or vocational degree.

Table 4.4 – Regression Results: Inflows of Refugees and Change in Unemployment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall		Youth		German		Non-German	
Mean Unempl. Rate	6.92		6.14		5.48		15.44	
(SD)	(3.20)		(2.94)		(3.27)		(6.18)	
refugees	-0.0003**	-0.0000	-0.0004**	-0.0001	-0.0009***	-0.0006***	0.0040***	0.0031***
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
EAEcap	-0.0002	-0.0001	-0.0001	-0.0001	-0.0000	0.0001	-0.0004	-0.0006
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0004)	(0.0005)
Observations	402	402	402	402	402	402	402	402
R-squared	0.0239	0.5110	0.0173	0.1098	0.0760	0.2147	0.0821	0.2415
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses.
Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2. The outcome variables are the unemployment rate for all workers (columns (1) and (2)), workers aged 15 to 25 (columns (3) and (4)), and workers who are (not) German citizens (columns (5) through (8)). The two main explanatory are the number of refugees allocated to a county during the 2014/15 refugee crisis and the number reception center (EAE) beds in the county (both per 100,000). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.

Table 4.5 – Regression Results: Inflows of Refugees and Change in Crime

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Crimes	All Cases		German Suspects		Non-German Suspects	
refugees	0.2511***	0.2300**	0.0291	0.0259	0.1416***	0.1434**
	(0.0950)	(0.0973)	(0.0187)	(0.0183)	(0.0544)	(0.0556)
EAEcap	-0.1825	-0.1795	-0.0348**	-0.0230	-0.0901	-0.0895
	(0.1359)	(0.1352)	(0.0171)	(0.0173)	(0.0958)	(0.0961)
Panel B: Street Crime	All Cases		German Suspects		Non-German Suspects	
refugees	0.0233	0.0128	-0.0062	-0.0074	0.0005	-0.0002
	(0.0244)	(0.0225)	(0.0042)	(0.0045)	(0.0028)	(0.0028)
EAEcap	-0.0250	-0.0271	-0.0104**	-0.0093**	0.0030	0.0029
	(0.0211)	(0.0212)	(0.0046)	(0.0046)	(0.0028)	(0.0027)
Panel C: Drug Offenses	All Cases		German Suspects		Non-German Suspects	
refugees	0.0077	0.0073	0.0107	0.0107	-0.0056*	-0.0052*
	(0.0115)	(0.0111)	(0.0073)	(0.0071)	(0.0029)	(0.0028)
EAEcap	0.0222***	0.0184**	0.0145**	0.0120**	0.0055**	0.0049**
	(0.0082)	(0.0080)	(0.0058)	(0.0056)	(0.0024)	(0.0023)
Panel D: Fare Dodging	All Cases		German Suspects		Non-German Suspects	
refugees	-0.0031	0.0005	-0.0037	-0.0022	-0.0038	-0.0011
	(0.0117)	(0.0120)	(0.0044)	(0.0047)	(0.0066)	(0.0066)
EAEcap	0.0203**	0.0214***	0.0020	0.0030	0.0196**	0.0183**
	(0.0083)	(0.0082)	(0.0032)	(0.0033)	(0.0078)	(0.0076)
Convariates	No	Yes	No	Yes	No	Yes
Observations	402	402	402	402	402	402
R-squared	0.0448	0.0699	0.0193	0.0891	0.0512	0.0679

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses.
Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2. In Panel A, the outcome variables are the number of crimes per population of 100,000 , the number of cases with German suspect(s) per population of 100,000, and the number of cases with Non-German citizen(s) as suspect(s) per population of 100,000. Offenses against immigration laws (e.g. unauthorized entry of German territory) are ignored. In Panel B, the outcome variables are the number of street crimes (bag-snatching, bike theft,...) with the same distinction between German and non-German suspects in columns (5) through (8). In Panels C and D the outcome variables are the number of drug-related offenses and fare-dodging offenses respectively. The two main explanatory are the number of refugees allocated to a county during the 2014/15 refugee crisis and the number reception center (EAE) beds in the county (both per 100,000). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.
R-squared statistics refer to estimates in Panel A.

Table 4.6 – Regression Results: Inflows of Refugees and Voting behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	AfD Vote Share			Election Turnout			Support for Incumbent Party		
refugees	0.002	-0.001	-0.002	0.004***	0.001	0.001	-0.012*	-0.003	-0.012**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.006)	(0.007)	(0.005)
EAE-Kap	0.000	0.000	0.000	0.001	0.001	0.001**	-0.004	-0.002	-0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.003)	(0.002)	(0.002)
Convariates	No	Yes	No	No	Yes	No	No	Yes	Yes
Observations	119	119	94	120	120	94	120	120	94
R-squared	0.010	0.470	0.541	0.054	0.396	0.555	0.029	0.256	0.455
Hesse	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No

Notes: $*{*}*/{*}*/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses. Each column reports coefficients and standard errors from a county level OLS regression. Columns (1) through (3) conduct a First-Differencing analysis as described by equation 4.2. That is, the difference between the AfD party's performance in 2016 regional elections and its vote share the 2013 federal elections is regressed on our two main explanatory variables (and covariates). Columns (4) through (9) repeat this for turnout and the share of the incumbent party. The 2016 elections were all state elections with the exception of the state of Hesse where votes were cast in municipal elections. The two main explanatory are the number of refugees allocated to a county during the 2014/15 refugee crisis and the number reception center (EAE) beds in the county (both per 100,000). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.

Table 4.7 – Placebo Regressions: Inflows of Refugees and Change in Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Unemployment				Crime		
	General	Youth	Non-German	German	All Crimes	Street Crimes	Drug Offenses
refugees	−0.0000	0.0001	0.0001	−0.0000	−0.2944***	−0.0694**	−0.0149
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0809)	(0.0291)	(0.0116)
EAEcap	−0.0001	−0.0001	0.0001	−0.0001	0.1384*	−0.0109	0.0104
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0794)	(0.0293)	(0.0102)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	402	402	402	402	402	402	402
R-squared	0.2267	0.0852	0.0425	0.2158	0.1119	0.0517	0.0224

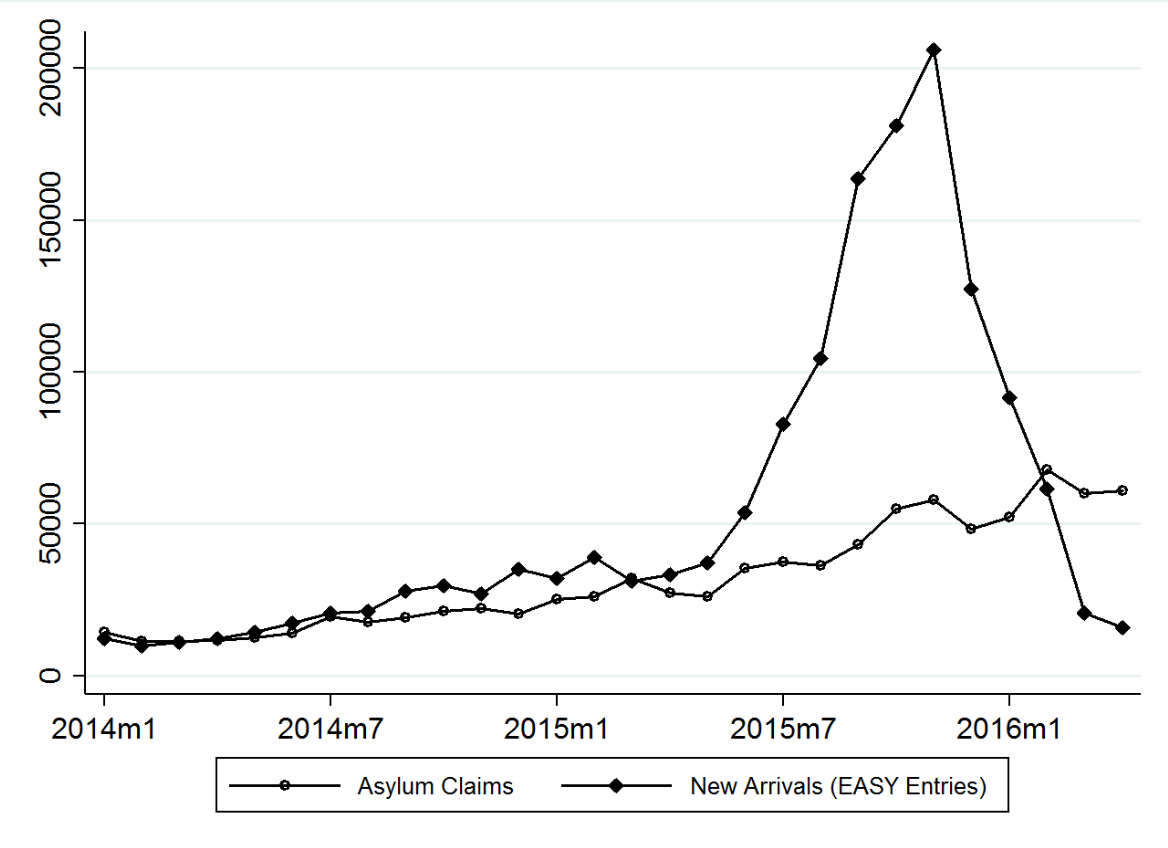
Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses. Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2, but based on data from 2013 and 2011 respectively. Refugee inflows and reception center (EAE) capacities were set to 2014/15 aggregates (both per 100,000). The outcome variables are the general unemployment rate, the unemployment rate for 15 to 25-year olds, the unemployment rate for workers who are not German citizens, the number of crimes, the number of street crimes, and the number of drug offenses (all three per 100,000 population). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.

Table 4.8 – Regressions Results: Effect of 2015 Inflows Relative to 2014 Inflows

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	Unemployment (1/2014 vs 1/2016)				Crime (2014 vs 2015)			
	General	Youth	Non-German	German	All Crimes	Street Crimes	Drug Offenses	Fare Dodging
$\triangle refugees_{2015-2014}$	−0.0000	−0.0003*	0.0029***	−0.0007***	−0.1366	0.0387	0.0005	0.0163
	(0.0001)	(0.0001)	(0.0009)	(0.0002)	(0.9992)	(0.0274)	(0.0114)	(0.0165)
$\triangle EAEcap_{2015-2014}$	−0.0001	−0.0002	−0.0005	−0.0001	0.6861	−0.0270	0.0121	0.0159
	(0.0001)	(0.0001)	(0.0005)	(0.0001)	(0.4778)	(0.0275)	(0.0089)	(0.0140)
Observations	402	402	402	402	402	402	402	402
R-squared	0.4116	0.0737	0.2719	0.4316	0.0624	0.0293	0.0152	0.0224
County-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

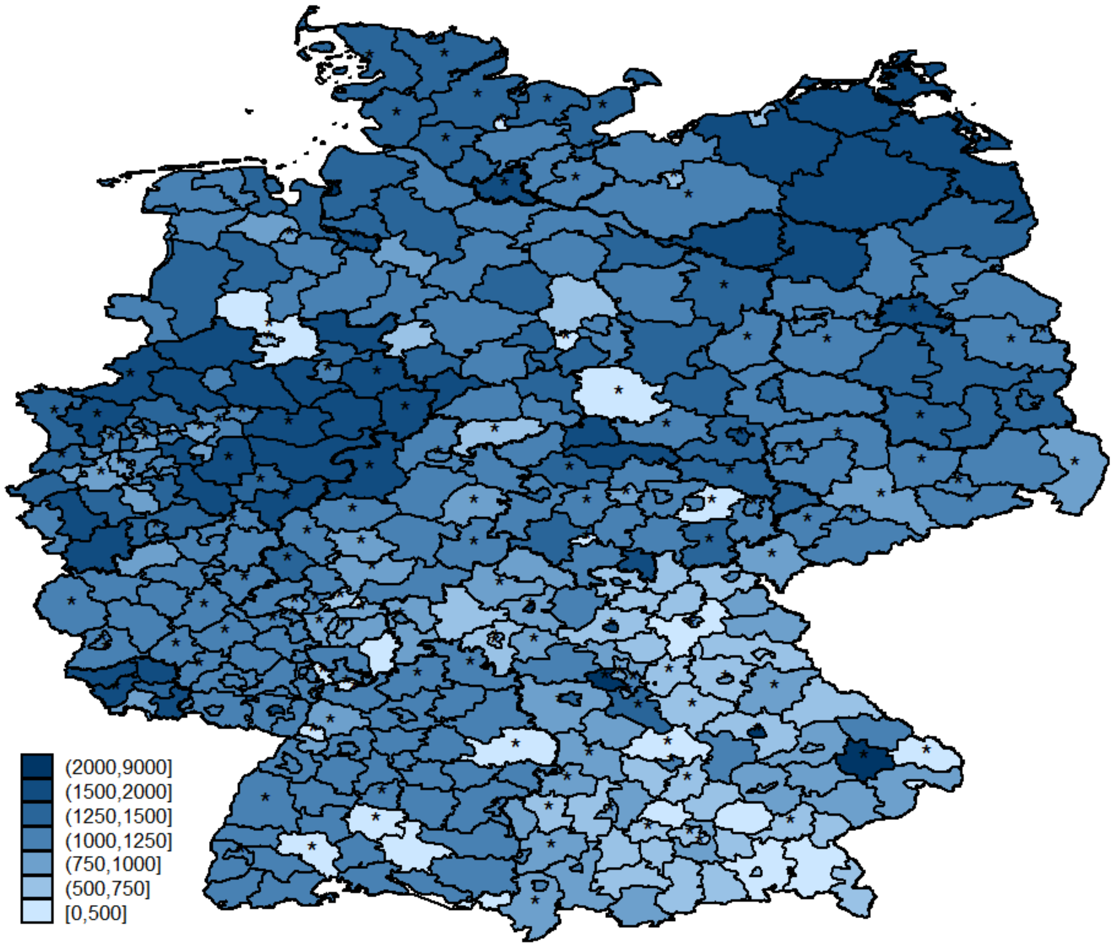
Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses.
Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2, but based on data from 2015/16 and 2014. In other words, we use the difference in 2015 and 2014 refugee inflows as the main explanatory variable. The outcome variables are the general unemployment rate, the unemployment rate for 15 to 25-year olds, the unemployment rate for workers who are not German citizens, the number of crimes, the number of street crimes, and the number of drug offenses (all three per 100,000 population). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.

Figure 4.1 – Refugee Arrivals and Asylum Claims Filed



Source: Federal Ministry of the Interior and Federal Office for Migration and Refugees
Notes: This graph plots the number of asylum applications that were filed and the number of new arrivals to Germany as they were entered into the federal registration system, EASY, between January 2014 and April 2016. The total for 2015 is 1,091,984 EASY entries, for 2014 it is 238,676.

Figure 4.2 – Refugee Allocations per County

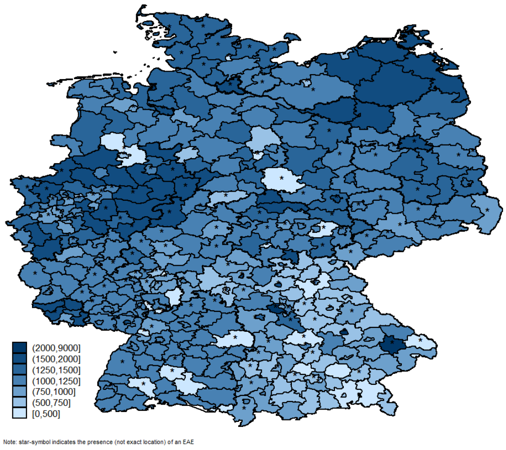


Note: star-symbol indicates the presence (not exact location) of an EAE

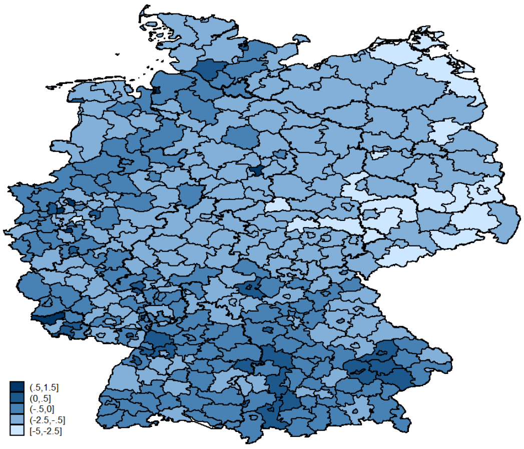
Source: State Ministries of the Interior or similar concerned state-level authorities
Notes: Map shows all 402 German counties and the influx of refugees into these counties per 100,000. Stars indicate the presence of a registration center (EAE).

Figure 4.3 – Refugees and Change in Outcomes by County

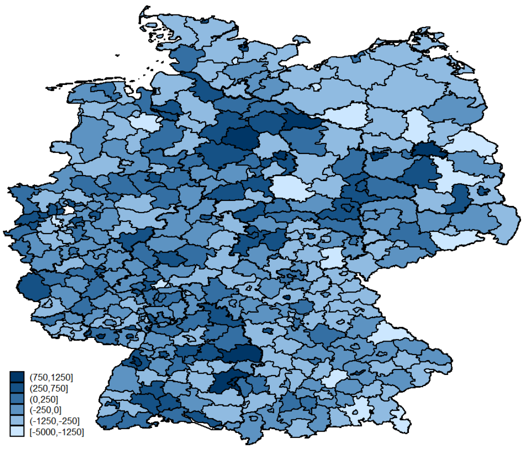
(a) Influx of Refugees



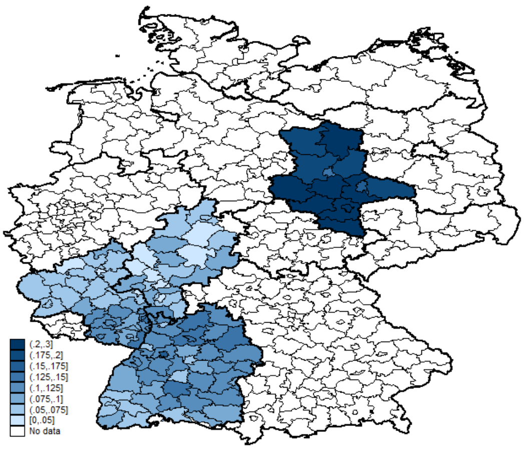
(b) Change in Unemployment Rate



(c) Change in Number of Crimes

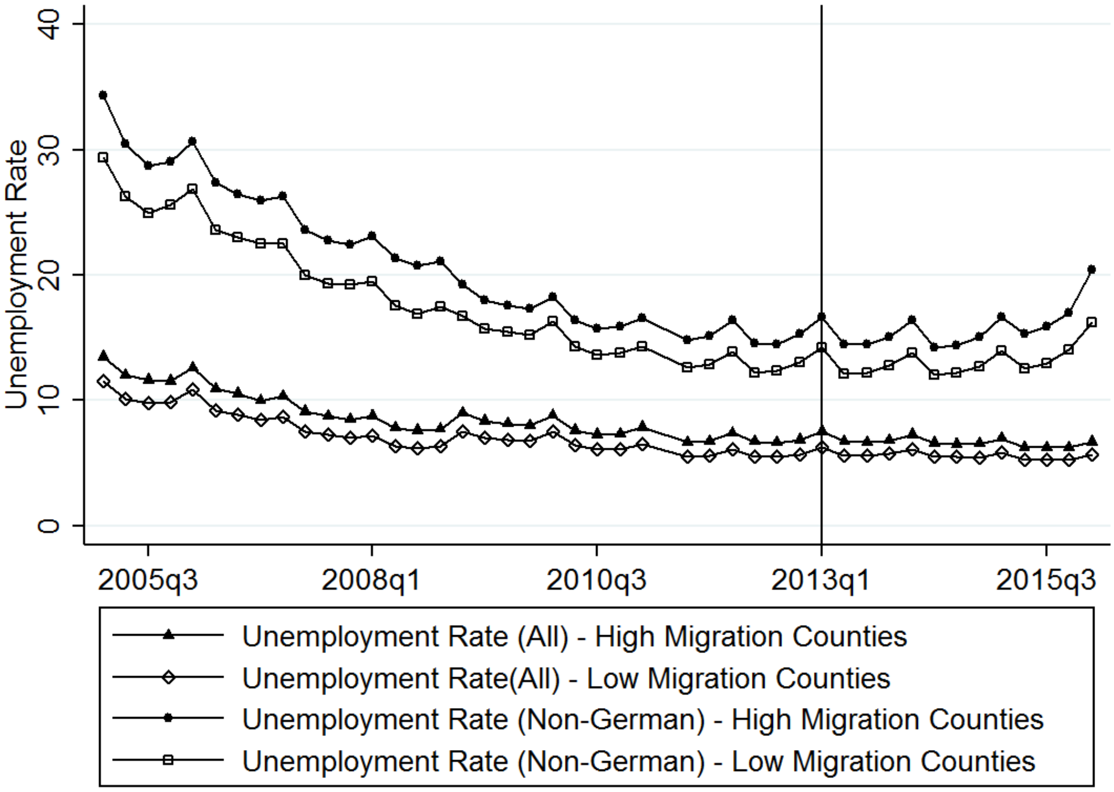


(d) Change in AfD Vote Share



Source: State Ministries of the Interior or similar concerned state-level authorities
Notes: Maps show all 402 German counties, the influx of refugees into these counties and changes in the main outcomes of interests between 2013 and 2015/16. Stars indicate the presence of a registration center (EAE). Note that the map on the top right is identical to Figure 4.2. Maps (a) and (c) are per 100,000 inhabitants.

Figure 4.4 – Unemployment Rates Over Time

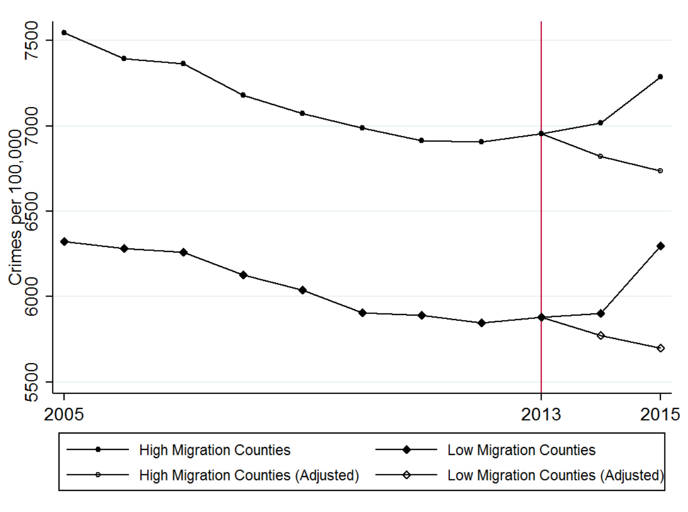


Source: Federal Employment Agency

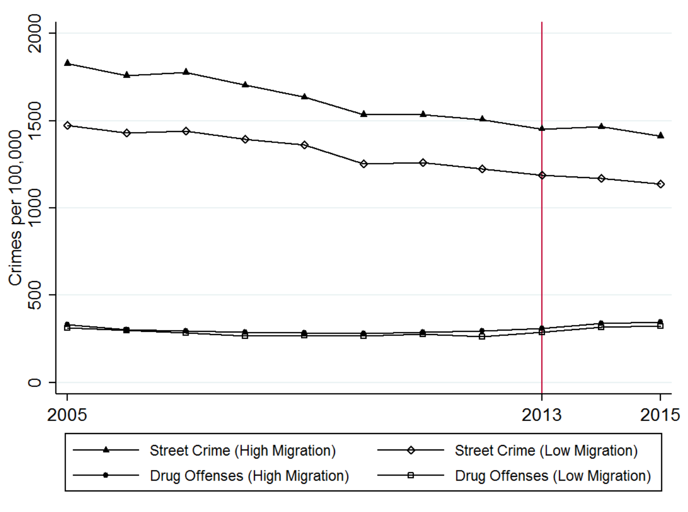
Notes: This figure shows quarterly unemployment rates (1/2005 - 1/2016) separately by low and high migration counties. High migration counties were allocated more than 1,305 refugees (per 100,000) or host a reception center (EAE) with at least 200 beds. The bottom two lines show the general unemployment rate, the top two lines show unemployment among the non-German population.

Figure 4.5 – Crime Rates Over Time

(a) All Crimes



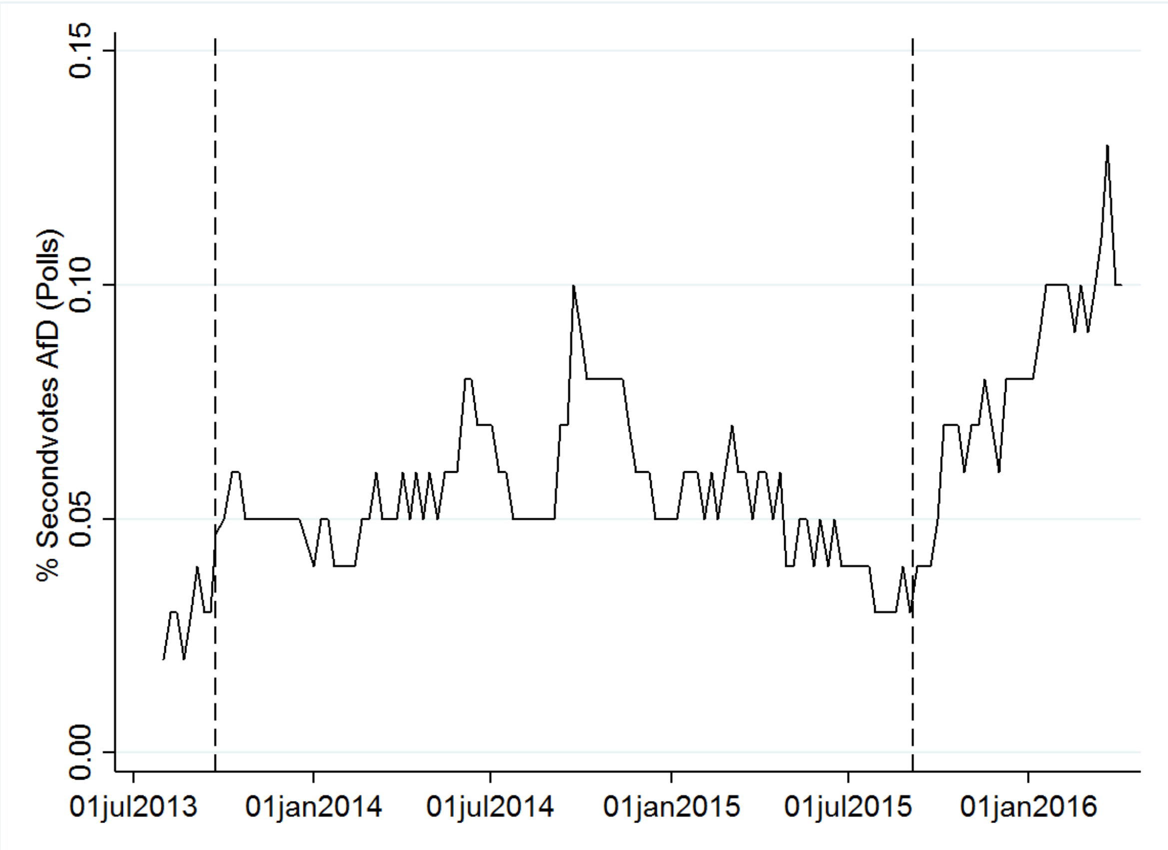
(b) Street Crimes and Drug Crimes



Source: Federal Criminal Police Office (BKA)

Notes: This figure shows annual crime rates (2005-2015) separately by low and high migration counties. High migration counties were allocated more than 1,305 refugees (per 100,000) or host a reception center (EAE) with at least 200 beds. The top two lines illustrate the number of street crimes (per 100,000), the bottom two lines show the number of drug related crimes (per 100,000).

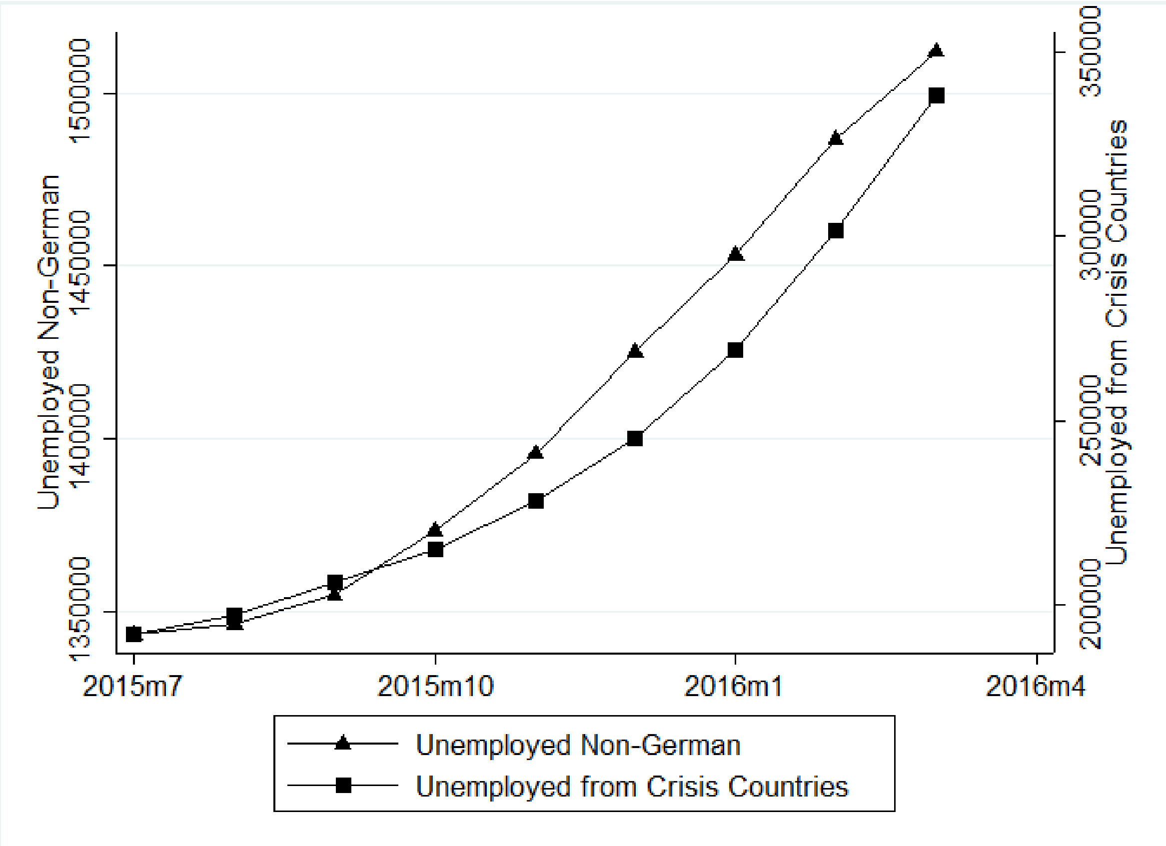
Figure 4.6 – National AfD Party Polls



Source: Forsa

Notes: These are national polls for the AfD party over time. The left vertical line is placed at the date of the latest federal election (22 September 2013) and the value at this point reflects the actual percentage of votes cast for the AfD party. All other measures of AfD popularity are based on polls contacted by the polling institute Forsa and are based on surveys of about 1,000 participants. The dashed vertical line to the right is placed on 5 September 2015 which is widely seen as the beginning of the refugee crisis.

Figure 4.7 – Number of Non-German Unemployed and Unemployed from Crisis Countries



Source: Federal Employment Agency

Notes: This graph plots the number of Non-German citizens who have registered for unemployment benefits with the Federal Employment Agency (left-handside y-axis). It also plots the number of citizens from the eight most common countires of origin for refugees (Syria, Iraq, Afghanistan, Iran, Pakistan, Nigeria, Eritrea, and Somalia) on the right-handside y-axis. Note that the data underlying this graph are based on a different definition of unemployment than the data in the previous graphs and tables. The data here include workers who are taking part in active labor market policy programs, such as requalifications and other government programs.

Table A2 – Main Regression using 2005-2015/16 Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Unemployment				Crime		
	General	Youth	Non-German	German	All Crimes	Street Crimes	Drug Offenses
refugees	-0.0006^{***}	-0.0006^{***}	0.0032^{***}	-0.0013^{***}	0.1463	-0.0126	-0.0073
	(0.0002)	(0.0002)	(0.0009)	(0.0003)	(0.1237)	(0.0425)	(0.0177)
EAE-Kap	-0.0002^{**}	-0.0002^{*}	-0.0004	-0.0001	0.0107	0.0110	0.0128^{**}
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0631)	(0.0139)	(0.0051)
Observations	4,422	4,422	4,422	4,422	4,422	4,422	4,422
R-squared	0.9452	0.9127	0.8566	0.9382	0.9558	0.9540	0.8058

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors account for clustering at the county level. Each column reports coefficients and standard errors from county level OLS regression based on data from 2005 to 2015/16. Regressions include full sets of county and year dummies. The outcome variables are the general unemployment rate, the unemployment rate for 15 to 25-year olds, the unemployment rate for workers who are not German citizens, the number of crimes, the number of street crimes, and the number of drug offenses (all three per 100,000 population).

Appendix: Jobs, Crime, and Vote - A short-run Analysis of the German Refugee Crisis.

Table A3 – Regression Results: Inflows of Refugees and Change in Unemployment Q2 2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall		Youth		Non-German		German	
refugees	-0.0005^{***}	-0.0003^{**}	-0.0007^{***}	-0.0004^{**}	0.0029^{***}	0.0023^{***}	-0.0013^{***}	-0.0009^{***}
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0007)	(0.0006)	(0.0003)	(0.0002)
EAEkap	-0.0002^{**}	-0.0002^{*}	-0.0003^{**}	-0.0003^{**}	-0.0007^{*}	-0.0009^{**}	0.0001	0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0004)	(0.0004)	(0.0001)	(0.0001)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Observations	402	402	402	402	402	402	402	402
R-squared	0.0633	0.4637	0.0542	0.2065	0.0653	0.2421	0.0715	0.1840

Notes: $*{*}*/{*}{*}/{*}$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses.
Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2. The outcome variables are the unemployment rate for all workers (columns (1) and (2)), workers aged 15 to 25 (columns (3) and (4)), and workers who are (not) German citizens (columns (5) through (8)). The two main explanatory are the number of refugees allocated to a county during the 2014/15 refugee crisis and the number reception center (EAE) beds in the county (both per 100,000). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.

Table A2 – Inflows of Refugees and Change in Unemployment - Separate results for number of refugees and EAE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: County Allocations Only	Overall		Youth		German		Non-German	
refugees	−0.0003**	−0.0000	−0.0003**	−0.0001	−0.0009***	−0.0006***	0.0040***	0.0033***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0008)	(0.0008)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Observations	402	402	402	402	402	402	402	402
R-squared	0.0151	0.5062	0.0146	0.1074	0.0760	0.2141	0.0809	0.2385
Panel B: EAE Capacities Only	Overall		Youth		German		Non-German	
EAE-Kap	−0.0001	−0.0001	−0.0001	−0.0001	0.0001	0.0001	−0.0008*	−0.0010**
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0004)	(0.0005)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Observations	402	402	402	402	402	402	402	402
R-squared	0.0056	0.5106	0.0011	0.1075	0.0014	0.1828	0.0057	0.1972

Notes: $*{*}*/{*}{*}/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses.
Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2, but only including one of our two main explanatory variables at a time. Panel A assesses the effect of number of refugees allocated to a county (per 100,000) during the 2014/15 refugee crisis without controls for (state-run) EAE capacities. Panel B assess the effect of the number of (state-run) reception center (EAE) beds in the county (per 100,000) without controlling for number of refugees allocated to a county. The outcome variables are the unemployment rate for all workers (columns (1) and (2)), workers aged 15 to 25 (columns (3) and (4)), and workers who are (not) German citizens (columns (5) through (8)). Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.

Table A3 – Inflows of Refugees and Change in Crime: Log-Level Specification

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Crimes	All Cases		German Suspects		Non-German Suspects	
refugees	0.000041***	0.000037**	0.000015*	0.000014	0.000127**	0.000121**
	(0.000015)	(0.000015)	(0.000009)	(0.000009)	(0.000053)	(0.000058)
EAEcap	0.000002	0.000000	0.000001	0.000001	-0.000013	-0.000010
	(0.000005)	(0.000005)	(0.000003)	(0.000003)	(0.000024)	(0.000025)
Panel B: Street Crime	All Cases		German Suspects		Non-German Suspects	
refugees	0.000058***	0.000054***	0.000022	0.000013	0.000142***	0.000130**
	(0.000018)	(0.000018)	(0.000025)	(0.000027)	(0.000051)	(0.000051)
EAEcap	0.000015***	0.000009**	-0.000005	-0.000008	0.000033**	0.000035**
	(0.000005)	(0.000004)	(0.000006)	(0.000007)	(0.000016)	(0.000015)
Panel C: Drug Offenses	All Cases		German Suspects		Non-German Suspects	
refugees	0.000038	0.000039	0.000034	0.000041	-0.000015	-0.000020
	(0.000030)	(0.000031)	(0.000029)	(0.000029)	(0.000066)	(0.000067)
EAEcap	0.000016	0.000017*	0.000007	0.000011	0.000060**	0.000056***
	(0.000010)	(0.000010)	(0.000010)	(0.000010)	(0.000025)	(0.000021)
Panel D: Fare Dodging	All Cases		German Suspects		Non-German Suspects	
refugees	0.000132**	0.000155***	0.000116**	0.000132**	0.000185**	0.000167**
	(0.000058)	(0.000055)	(0.000058)	(0.000053)	(0.000080)	(0.000083)
EAEcap	0.000043**	0.000031	0.000026*	0.000015	0.000052*	0.000051*
	(0.000021)	(0.000022)	(0.000015)	(0.000016)	(0.000029)	(0.000030)
Convariates	No	Yes	No	Yes	No	Yes
Observations	402	402	402	402	402	402
R-squared	0.026884	0.076150	0.007195	0.053387	0.032715	0.057718

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses.
Each column reports coefficients and standard errors from a county level OLS regression as shown in equation 4.2. In Panel A, the outcome variables are the log number of crimes, the log number of cases with German suspect(s) and the log number of cases with Non-German citizen(s) as suspect(s), all per 100,000. Offenses against immigration laws (e.g. unauthorized entry of German territory) are ignored. In Panel B, the outcome variables are the log number of street crimes (bag-snatching, bike theft,...) with the same distinction between German and non-German suspects in columns (5) through (8). In Panels C and D the outcome variables are the log number of drug-related offenses and fare-dodging offenses respectively. The two main explanatory are the number of refugees allocated to a county during the 2014/15 refugee crisis and the number reception center (EAE) beds in the county. Covariates are all county-specific and include housing vacancies (per 1,000 inhabitants), per capita GDP (in €), average age, share of population with migration background, and share of population with a college or vocational degree, and the county population. All covariates except for population are as of the Zensus 2011.
R-squared statistics refer to estimates in Panel A.

5 Integrating Refugees: How Effective is a Residency Obligation?

5.1 Introduction

While there has been extensive research and discussion on the net effect of migration on the host-country, no consensus has been reached on which factors foster integration. Since immigration is increasing again all over Europe, this topic is also back on the political agenda. With the arrival of almost 900,000 refugees in Germany in 2015, the government found itself with the challenge to organize not only the asylum application process and initial housing, but also to come up with a plan for long term integration of accepted refugees. One political instrument was the integration law, including the possibility for states to install a residency obligation (Integrationsgesetz (InteG), 2016). The main scope of this paper is to analyze the effect of the implementation of a residency obligation for accepted migrants in four out the 16 federal states in Germany on employment of accepted refugees and criminal behavior.

For labor migration, the ability of the labor market to absorb a potential supply shock, is less of a concern (Fitzgerald et al., 2012). In the same line, criminal behavior of working migrants is hardly found (Bell et al., 2013b). However, in the case of refugee migration the federal employment agency finds lower labor force participation of refugees in comparison with other migrants, especially in the first years after arrival (Brücker et al., 2015). In addition, increasing crime rates are found in the academic literature for refugees rather than working migrants (Bell et al., 2013b).

In order to facilitate in a more efficient way, the German government installed a new integration law in 2016. It requires accepted refugees to stay in the federal state where their asylum claim was processed. Furthermore, it enables states to put in place residency obligations on county level. This has been done before in the context of ethnic Germans integration to Germany in the 1990 (Aussiedler). Assigning residency has mostly been motivated by prevention of clustering and the negative effect of migrant enclaves. Generally, this has been evaluated as a successful measure (Bauer and Zimmermann, 1997). However, recent studies point to a differential effect depending on pre-existing conditions within regions (Piopiunik and Ruhose, 2017). Therefore, a detailed analysis of the connection between the arrival of asylum seekers, their regional distribution after a positive asylum decision, and their prospects of integration is of highest interest. This paper makes use of the fact that some states implemented a residency obligation, while others did not. This gives rise to an evaluation of the effectiveness of an allocation rule for migrants. This is especially important, as the net effect of migrant clustering is ex-ante unclear from both, theoretical and empirical perspective. On the one hand, it might foster integration due to network effects and information on available jobs Bertrand et al. (2000). On the other hand, it might hinder labor market integration due to lacking need to acquire country specific skills, such as language Warman (2007).

My contribution to this literature stems from the analysis of a huge and temporary increase in asylum seekers arriving in Germany in 2015 and their regional allocation after a positive asylum decision. As the German government put in place a residence law in 2016, all accepted migrants are obligated to live in the states where their asylum claim was processed. Furthermore, some states decided to assign specific residence to the migrants on the county level. This law was subject to a heavy debate. Some states and politicians welcomed it as a necessity to meet the challenge of mass migration. Bavaria for example immediately announced to make use of the rule. Migrant organizations and to some extend also the research center of the federal employment agency criticized it could harm integration and hinder access to the labor market, as search costs are increased. Other states did not see a need for further governing migrant allocation, as most of them would stay in the area they were initially assigned to. This framework gives rise to a classic difference in difference (DiD) analysis of the allocation rule with a clear separation between treatment and control group. While the allocation of ethnic Germans is generally seen as a success, no study explicitly investigates the causal effect of a residency obligation. Therefore, this paper provides a first evaluation of the effectiveness of an allocation rule for newly arriving refugees in Germany.

The baseline analysis consists of descriptive evidence on the moving activities of accepted migrants in Germany on the county level. The key question is to what extent resettlement after the asylum decision is driven by favorable regional conditions, such as infrastructure and labor market opportunities, pre-existing clusters of migrants, or even the location of the initial distribution. The second step is a DiD analysis of migrant allocation on crime rates and employment of accepted migrants. As data availability on migrants is a key issue, I use several datasets and compare results. I find that the residency obligation does not significantly affect overall crime rates. Meanwhile, there is a small but significant positive effect on employment of accepted refugees.

The remainder of the paper is structured as follows. Section 5.2 gives an overview over the existing literature on immigrant clustering and it's potential effects on integration, measured by crime and employment. Section 5.3 describes the allocation process as well as potential relocation activities. Section 5.4 lists the data and it's sources, while section 5.5 lays out the empirical methodology. Section 5.6 presents the key results and additional evidence on regional heterogeneity. Section 5.7 describes prospects for the evaluation of the residency obligation in the future. Finally, section 5.8 concludes.

5.2 Immigrant Enclaves and Integration in the Literature

The effect of migration on employment and crime has been subject to extensive research. From a theoretical perspective, the effect of migration on the labor market depends on the skill composition of immigrants, its transferability to the host-country, and the match with job vacancies. Hence, the ex-ante result is not clear (Borjas, 1999). Therefore, empirical studies find opposing results, depending on the context of the study. The general debate can be summarized by a prominent example of an ongoing controversial in the literature. On the one hand, Borjas (2003b) argues for negative effects of migration on wages and employment of natives.[1] On the other hand **?** tends to find zero effect.[2] Dustmann et al. (2016) examine potential reasons for varying results and argue that static assumptions on labor supply elasticities and downgrading of immigrants play an important role when measuring relative effects of migration. Absolute effects, however, seem to be invariant to theses biases. In addition Dustmann et al. (2012) report heterogeneous effects along the distribution of wages. Ceritoglu et al. (2017) exploit the consequences of the civil war in Syria and find considerable effects on employment and outcome of natives, especially in the informal sector. For the case of Germany, Glitz (2012b) analyses the allocation of ethnic Germans from Russia and reports a significant displacement effect of migrants on native workers, mostly depending on the initial skill composition of the region.

Compared to employment, the body of literature focusing on criminal consequences of increased migration is relatively small. Until recently, the literature found little to no evidence for a direct link between migration and increasing overall crime rates. Bianchi et al. (2012b) studied migration to Italy from the Soviet Union and the Balkan in the 1990s. While they find a significant effect on robberies, overall crime rates remained

[1] See also **?** for a more theoretical approach and Borjas and Monras (2017) for empirical evidence on refugee supply shocks.

[2] See also Card (2009, 2012b).

mostly unaffected. Bell et al. (2013b) focus on migrant workers and refugees from those countries that joined the EU in 2004 and their effect on crime rates in the UK. They only find significant results for refugees and only for property crime. Piopiunik and Ruhose (2017) studied migration and allocation of ethnic Germans from the Soviet Union in the 1990s. Their paper is the first contribution to the literature that explicitly takes into account the role of local economic heterogeneity in the relation between migration and crime. In contrast to most other studies, they find that migration significantly increases crime. The magnitude of their results depends strongly on local labor market conditions, pre-existing crime levels, and larger share of foreigners. However, as these immigrants are ethnic Germans, they do not fear the threat of removal in case of criminal behavior, as they were granted German citizenship immediately.

The main goal of a residency obligation is to prevent the rise of large immigrant enclaves. This is mainly done for two reasons. First, clustering is expected to hinder integration. Second, single administrations might be overwhelmed if a sufficiently large number of migrants decides to locate in the same county. The latter also plays a role in terms of the willingness and ability of the hosting society to welcome larger shares of immigrants. However, most studies rely on comparing different groups of migrants, i.e. refugees vs. working migrants, as usually all migrants of one group are subject to a residency allocation (Bell et al., 2013b; Piopiunik and Ruhose, 2017).

In economic theory, an ethnic enclave is defined as a clustering of an ethnic minority in a sufficiently small region (Clark and Drinkwater, 2002). If such an enclave is sufficiently large, it can be interpreted to work in terms of information and norms for its inhabitants (Bertrand et al., 2000; Portes and Shafer, 2007). On the information side, a network provides the inhabitants with information on potential job offers or other sources of income, such as social security (Bayer et al., 2008; Bertrand et al., 2000). Thereby, immigrants can avoid initial discrimination in the host country labor market. Moreover, networks can also affect migrant's labor market integration via social norms. On the one hand, direct effects stem from work ethics and self-employment. Indirect effects, on the other hand, run through educational attainment or family values (Damm, 2009b).

Acemoglu and Angrist (2000) establish a link between human capital externalities and the quality of a neighborhood. Hence, the quality of an enclave plays a key role in the accumulation of skills. Moreover, it affects the investment in country specific human capital, especially language skills (Warman, 2007). This channel potentially hinders labor market integration in the host country as human capital of the enclave is less transferable. While the enclave might constitute a labor market by itself (Portes and Shafer, 2007), it is expected to pay lower wages due to monopsony power of employers (Borjas, 2000).

Another important channel is the spatial mismatch theory (Kain, 1968). First of all, the spacial mismatch between residential areas of immigrants and job vacancies results in lack of information on jobs and higher commuting cost. Moreover, prejudice is fostered among potential employers, stemming from a lack of interaction with immigrants [3]. Both arguments potentially hinder labor market integration of immigrants.

Thus, the network effect of an enclave is ex-ante unclear and most likely driven by the quality of the enclave. Similarly to theoretical arguments, the empirical literature finds ambiguous results. Some studies find positive effects of enclaves on the probability of employment and self-employment (Borjas, 1986; Lancee, 2010). Others report negative effects (Clark and Drinkwater, 2002; Pedace and Rohn Kumar, 2014). A potential reason for the heterogeneity in empirical findings is the way enclave composition and quality affects labor market integration. This could be different for ethnic groups, skill level of immigrants, average education and income levels of immigrants and time frame of residence (Schaffner and Treude, 2014).

For Germany, Schaffner and Treude (2014) find negative enclave effects on both, employment and wages using zip code data level. Their results are even stronger when sorting is taken into account. No effect is found by Kanas et al. (2012). However, their approach is based on the nationality share on the states level, which is likely to omit potentially large heterogeneity in smaller regions. Bauer and Zimmermann (1997) analyze the effect of enclaves in the context of ethnic Germans from the former Soviet Union. They find positive effects on employment.

Directly related to the labor market opportunities of immigrants is the potential for criminal activities. When it comes to the effect of enclaves and clustering on crime, there has been surprisingly little research. In the same line as for employment outcomes, theory suggests ambiguous effects. A potential channel is the social control exercised in enclaves. On the one hand, enclaves can lower crime rates through protective and revitalizing effects of immigrants settling in spatially concentrated neighborhoods. On the other hand, segregation can foster criminal behavior, particularly in disadvantaged contexts (Feldmeyer et al., 2015).

[3] See Steinmayr (2016) for empirical evidence on the theory of exposure and migrant acceptance.

Empirically, Bell et al. (2013b) finds a declining effect of immigration on crime, but only if a enclave reaches a certain size threshold. Cutler et al. (2008) observes positive effects only for highly educated groups of immigrants. Using a randomized control trial, Kling et al. (2005) show that relocation to low-poverty neighborhoods is beneficial only for women.

Overall, the effect of enclaves and residency obligations are theoretically ambiguous. Empirical research finds opposing results, depending on the context, such as enclave quality, composition of immigrant groups, and pre-existing composition.

5.3 Allocation and Relocation: Refugee Migration to Germany

The mass-migration to Germany in 2015 challenged the asylum process to some extent.[4] While the system was working well in general, there were short-term capacity problems that led to deviations from the obligated quotas to host asylum seekers. Allocation to counties was extended to six months or more in order to allow for more time for the asylum claim to be processed. Especially the federal migration office faced problems in processing asylum claims in a timely manner. Therefore, it took until 2016 for the majority of cases to be decided (Federal Office for Migration and Refugees, 2017). Figure 5.1 shows the development of potential labor force members, asylum aid receivers, and registered non-Germans from the eight most prominent refugee countries and makes clear that the potential labor force, in contrast to overall number of foreigners, only started to strongly increase in 2016. Hence, labor market integration started mostly since then.

Overall, acceptance rates are the highest for Syrians, followed by Iraqi and Afghans (Federal Office for Migration and Refugees, 2017). After a positive asylum decision, migrants have full access to the German labor market. However, since August 2016, the German states have the right to dictate the place of residency (Integrationsgesetz (InteG), 2016). The German government installed the so called Integrationsgesetz (InteG) in August 2016. It requires persons with positive asylum decision granted after January 1st 2016 to have their residence in the state where the asylum claim was decided for a period of up to three years. Non-compliance leads to a loss of social benefits. The residence obligation can be levied for persons with employment or for unique reasons upon request. Furthermore, it entitles states to execute own residence

[4]For a detailed description of the asylum process, see Gehrsitz and Ungerer (2016).

orders, e.g. requiring accepted asylum seekers to stay in a county based on a quota.

Baden Wuertemberg, Bavaria, and Northrhine-Westphalia all declared their willingness to adapt such a regulation and executed corresponding orders shortly after the law was official. Saxony Anhalt followed with a similar rule.[5] However, as it only decided in early 2017, I exclude it from the treatment group in the DiD analysis. The other states did, up to now, not install any residence obligation. Table A1 summarizes the current obligation scheme for all states. States without a residency obligation stated that accepted refugees are likely to stay in the region of their initial distribution. Hence, no further allocation scheme would be needed.

So far, all states use a quota based on population in the county. This distribution is based on a similar quota compared to the initial allocation of asylum seekers. However, as acceptance rates vary between nationalities and regions, a new allocation is required in order to ensure an even distribution based on population.

With those states which did not enforce an allocation policy, I analyze different potential determinants of migrant resettlement in Germany. First, I use the initial distribution of asylum seekers during the asylum process as an explanation for their final settlement decision. As the asylum decision takes some time, people may start building themselves a new home in the area of initial arrival. This argument has been made by some of the states that did not see the need for an allocation law. However, as the initial distribution was mainly based on population, this might not be an optimal choice in terms of integration conditions and infrastructure. Second, I examine pre-existing clusters of migrants from similar regions (Schaffner and Treude, 2014). Finally, favorable infrastructure may play a role for the settlement of new migrants. Therefore, I use labor market conditions, housing availability, schools, and hospitals as indicators for infrastructure.

Until 2009, there had already been a resettlement law in place in Germany.[6] It obligated ethnic Germans mainly from the Soviet Union to stay in certain areas. Punishment was mainly executed via reducing social benefits. To this respect, the rules are similar to those in place in some states of Germany since 2016. The general assessment

[5]One might be concerned with the choice of states depending on the political party in office. In Bavaria, the CSU was in office. In Baden-Wuertemberg, a coalition of the green party and the CDU decided upon the residency obligation. In Northrhine Westfalia, a coalition of social democrats (SPD) and green party ruled. Finally, in Saxony Anhalt, a coalition of CD, SPD, and geen party has been in office. Hence, there is no clear trend for a single politically party deciding on the residency obligation.

[6]Piopiunik and Ruhose (2017) and Bauer and Zimmermann (1997) use this allocation law in order to draw evidence on employment and crime.

of the previous resettlement by policy makes was positive (Federal Office for Migration and Refugees, 2007). Hence, it might serve as a comparison scenario in order to form hypothesis on the potential effects of a new resettlement law.

5.4 Data

For the empirical analysis, I make use of administrative data that provide detailed information on asylum seekers, county characteristics, unemployment, and crime rates. The data on initial distribution of refugees was collected by Gehrsitz and Ungerer (2016) by addressing the administration of each of the 16 German federal states. They provided information on the allocation of refugees to counties and the capacities of reception centers.

The federal statistical office hosts a regional database (Regionaldatenbank Deutschland). It provides detailed information on county characteristics, including gross domestic product (gdp), share of housing vacancies, age and education composition of the population, share of population with migration background, number of schools, hospital beds and population size. Furthermore, it also hosts data from the central register of non-natives (Ausländerzentralregister, AZR) on the number of people by home country and migration status. Furthermore, the federal statistical office offers data on the number of individuals currently receiving asylum benefits.

In order to measure immigration on the county level in 2016, there exist tow potential datasets. AZR registers all foreigners with information on legal status and nationality. However, the information included is subject to revision and not necessarily representative in most recent years, due to high fluctuation. Therefore, I use data from the employment agency on potential labor force members (gemeldete erwerbsfähige Personen) from the eight main refugee countries (MRC).[7] This data comprises of all unemployed people, seeking for employment, and those who receive social benefits and are hence potential members of the workforce. In addition, the labor office also makes data on employed migrants available. Thereby, I get a proxy of all people willing to settle and integrate into the German labor market. Moreover, the employment agency provides data on unemployed and employed persons on county level.

Finally, I use data on committed crimes and number of suspects from the federal criminal office. The data not only includes overall numbers off crimes, but also differ-

[7] These are Afghanistan, Eritrea, Iraq, Iran, Nigeria, Pakistan, Somalia, and Syria.

entiates between subgroups of criminal activities, such as violence, street crime, fare-dodging, drug crimes, assault, pick-pocketing, and violations of asylum law. Thereby, I am able to exclude violations against asylum law for some of the analysis, as they make up for a increasing number of crimes committed by foreigners.[8] Thereby, I follow Gehrsitz and Ungerer (2016) in their definition of overall crime and its subgroups.

Comparing data for migrants in the context of refugees is not straightforward. Both, the register of foreigners and the employment agency provide data on migrants from the main asylum countries. In order to identify individuals or groups as actual refugees, one would have to link these datasets to data from the Federal office for Migration and Refugees (BAMF). However, this is, by law, not allowed. Therefore, as most of the migrants from the eight main refugee countries are actually refugees, I use these figures as a proxy. Focusing on Syrians as a sensitivity check gives further credit to the analysis, as they have an acceptance rate for their asylum claim of over 95 percent and migration from Syria was rare prior to 2014.

Table A2 shows the stock of migrants from the eight main refugee countries per 100.000 population from two datasets as well as the difference to the previous period in order to account for pre-existing migrants. Overall, I find that the AZR captures slightly less migrants compared to data from the BA. This raises some precautions w.r.t the data quality of the AZR during times of mass-migration. While the overall number of registrations in the AZR match the overall refugee numbers quite well, the number of accepted refugees in the AZR is rather low. Therefore, one might assume that it takes some time until the legal status in the AZR is changed after a positive asylum decision. In addition, it becomes clear that counties in states that exercise a residence obligation did receive slightly less migrants per 100,000 population compared to those counties in states without such an obligation.[9] In contrast, states which installed a residency obligation seem to have distributed migrants more equally, as the standard deviation of migrants is lower for counties in these groups. In addition, I find stronger clustering prior to the refugee inflow using data on all migrants from 2011 as well as the subgroup of main refugee countries.

Table A3 lists descriptive statistics of key variables overall and separately by both groups of states. While I find that countries with and without residence obligation do not differ significantly in terms of refugees migrants, this is not always the case for county specific characteristics. Both groups of counties are similar in terms of average

[8]Technically, every asylum seeker commits a crime against residency law when first entering Germany.

[9]A potential explanation could be differences in acceptance quotas between states with and without residency obligation.

population. Also, infrastructure indicators, such as hospital beds and schools, do not statistically differ from each other. However, in terms of economic power, measured by GDP per capita and unemployment, counties in states with residence obligation are better of. Interestingly, this does not imply a higher rate of job vacancies listed with the federal employment agency in these counties. Moreover, there seems to be a higher rate of housing vacancies in counties without residence obligation. This finding is in line with the better economic situation in counties without residence obligation. The overall unemployment rate of foreigners is smaller in residence obligation counties, while employment rates from the main refugee countries are similar. Finally, there are higher pre-existing shares of foreigners and non-Germans in counties with residence obligation.

5.5 Empirical Model

The basic idea is to use the fact that some states use a residence obligation on county level for accepted asylum seekers while others do not. This gives rise to a classic difference in difference strategy. Thereby, counties in states with an residence obligation serve as a treatment group, while those counties in states without a residence obligation are the control group. The time of treatment is 2016 and I use 2015 as the pre-treatment period for now. In such a framework, it is possible to study the causal effect of an allocation mechanism based on a residence law on the integration of accepted refugees. As the most prominent concerns by the public are crime and employment, I study the effect on overall crimes per 100,000 inhabitants, selected sub-groups, and employment for accepted migrants from the eight main refugee countries per 100,000.

In order to measure the potential effect of the allocation rule on crime, I estimate the following DiD equation:

$$(crime/pop)_{it} = \alpha_{it} + \beta D_{2016} + \gamma TS + \delta TS \times D_{2016} + \theta X_{it} + \epsilon_{it}, \tag{5.1}$$

while the relevant equation for employment is:

$$(emloyed_{mig}/pop)_{it} = \alpha_{it} + \beta D_{2016} + \gamma TS + \delta TS \times D_{2016} + \theta X_{it} + \epsilon_{it}, \tag{5.2}$$

where TS is a dummy indicating whether a county lies within a treatment state, X_{it} is a vector of county characteristics[10], and D_{2016} is a dummy indicating the treatment period. If the assumptions underlying the DiD estimator are met, the coefficient δ gives

[10]For the main analysis, I use the unemployment rate as a control variable. Due to data availability, other covariates are only available until 2015.

the effect of the introduction of a residence obligation on crime rates (employment of migrants) of counties in treatment states.

For this empirical strategy to be true, two fundamental assumptions have to be met. First of all, the stable unit treatment value assumption (SUTVA) requires treatment of one area not to affect another area. In the case of a residency obligation on state level, this might impose a potential threat, if immigrants could move into another state where the obligation is not in place. However, the federal integration law requires immigrants to stay in the state where the asylum claim was decided. Moreover, the treatment is not supposed to vary in quality. In all states that implemented the residency obligation, the law is applied to all immigrants with a positive asylum decision starting January 1st. As Lower Saxony did only decide upon implementation in early 2017, I drop it from the treatment group for now. Hence the SUTVA is convincingly satisfied. Secondly, the crime rates of counties treatment and control states need to have the same pre-trends. Otherwise, the estimation is biased to the extent that the effect of migration and residence obligation is mixed with already diverging crime rates. Figure 5.4 depicts the time series for overall crime rate for both groups of counties. At a first glance, counties in the non-residence obligation feature substantial higher crime rates compared to their counterparts in states with an obligation. Nevertheless, it is fair to argue that both groups share the same trend of declining crime rates over time until 2015. Figure 7 in the appendix gives further evidence on the stability of time trends over the last years. Similarly, figure 5.5 shows parallel time trends in employment from MRC prior to the introduction of a residency obligation. While it is not clear from figure 5.5 that there is any difference in the development since 2016, this only becomes clear when looking at the difference of both time series in Figure 8. While the effect is small in size, I find a sharp relative increase in employment after introduction of the residency obligation in the corresponding states.

As I am not only interested in the overall development of crime and employment under both regimes, I explicitly take migration patterns and its potential effect on crime and employment into account. Hence, I derive a difference in difference in difference estimator that exploits both, the treatment in terms of a residency obligation and the way migration affects crime under both allocation schemes. This gives rise to the following equation:

$$
\begin{aligned}
(outcome/pop)_{it} = \alpha + \beta_1 TS + \beta_2 D_{2016} + \beta_3 mig_{it} + \gamma X_{it} + \delta D_{2016} \times TS \qquad (5.3) \\
+ \theta TS \times mig_{it} + \psi D_{2016} \times mig_{it} + \phi TS \times D_{2016} \times mig_{it} + \epsilon_{it}. \qquad (5.4)
\end{aligned}
$$

Ultimately, one is interested in the effect migration has on crime rates under both regimes. In this context, the coefficient ϕ gives the desired treatment effect of an residence obligation on the relation between migration on crime. Similar to the classic DiD estimator, the pre-trend for high and low migration counties under both regimes have to be similar. Fig 5.6 provides justification to this assumption.

5.6 Results

As a first step, I describe potential determinants of migrant resettlement. In the context of counties in states without residency obligation, the results can actually be interpreted as a directly related to the allocation choice of migrants, as they are free to move within the state. For those states with a residency obligation, however, the results are merely a consequence of the external allocation process.

The second step comprises of an analysis of the effect of the residency obligation on employment and crime.

5.6.1 Main Findings

The initial distribution of refugees to states took place along population and tax revenue. Within state, distribution to counties was governed mostly by population. On the one hand, states that did not make use of the residency obligation stated that migrants are likely to stay in the area where they had been assigned in the first place and hence see no need for regulating allocation. States with an residency obligation in place, on the other hand, argued that migrants tend to favor metropolitan areas and the initial distribution was not efficient in terms of capacity for integration.

In fact, it is observed that the assigned refugees in 2015 are a strong predictor for potential labor force members and registered migrants from the MRC in counties w/o residency obligation, while it is not significant and smaller by the factor 10 in counties with (A4). Housing vacancies are a key factor for migration and integration. I find that migrants in both groups tend to settle more likely in counties with higher share of vacant homes. States with a residence obligation tend to perform better at allocating migrants to counties with relevant infrastructure compared to their counterparts in states w/o obligation. Hospital beds are a positive predictor for migrant resettlement and coefficient for schools is similar, but insignificant. In counties with residency obligation, however, more schools are a negative predictor for migrant resettlement.

Interestingly, both schemes allocate more migrants to counties with higher overall unemployment rate. The effect is twice as large in counties with an residency obligation. Rural areas tend to have higher unemployment rates and more housing available in general compared to metropolitan areas. As states with a residence obligation are interested in an even distribution across the state, relatively more migrants are allocated in rural areas with higher unemployment. Table A2 in fact shows that there is a tendency for stronger clustering in counties w/o residency obligation. For these counties, vacant jobs, a higher fraction of young citizens, and a higher share of population with migration background are positive predictors of migrant resettlement. The same characteristics also apply to metropolitan areas. Hence, it seems that migrants in states w/o residence obligation tend to rather settle in urban areas. The coefficient on urbanization points in that direction, being insignificant, however.

The key question arises directly from these observations. Namely, which allocation scheme does a better job at providing opportunities for integration. Therefore, I continue to analyze the differential effect migration has on crime and employment with and without a residence obligation.

The results of an ols regression of crime on migration are shown in Table A5. They show a general connection of crime and migration. However, these coefficients cannot be interpreted in a causal way for several reasons. First, the allocation rule provides a cut in the data that is not captured by such an analysis. Second, and more important, a simple ols framework treats the location choice of migrants as exogenous. However, as migrants are able to actually choose their place of residence in 12 out of 16 states, there is no particular reason to believe this to be the case. If migrants tend to move to areas with lower crime rates, my results will be biased. While all the results for all three sources of migrant data point in the same direction, they are most significant for potential labor force members. One reason is the fact that this data provides the cleanest measure of accepted refugees at this point in time.

As I am ultimately interested in the effect of the residency obligation on criminal activities, I continue with a DiD analysis as described by equations 5.1 and 5.2. Table A6 shows the effect of introducing a residency obligation in Bavaria, Baden-Wuertemberg, and Northrhine Westfalia. As already mentioned, Saxony Anhalt is excluded from the treatment group, as they only decided upon the residence obligation in early 2017. The relevant coefficient δ form equation 5.1 is resembled by DiD in table A6. I find a negative but insignificant effect of the allocation rule on overall number of committed crimes per 100,000 population. The coefficient indicates 15 crimes per 100,000 less in counties from states with a residency obligation. Hence, the implementation of a residency obligation has, so far, not significantly affected overall crime rates.

Overall crime rates potentially hide heterogeneous effects for selected criminal activities. Therefore, table A8 shows the results for selected crime subgroups with and without covariates. While coefficient are generally negative, It becomes clear that no a single crime subgroup stands out significantly.

With a DiD analysis, I am only able to identify an overall effect, but have no insight into the mechanism that shapes the relation between migration and crime. Therefore, I apply a more sophisticated empirical strategy in order to explicitly take into account differentials in migration intensity in both groups of counties. Table A7 shows the result of an estimation in the sens of equation 5.3. The coefficient DiDiD (ϕ in equation 5.3) measures the difference between the effect of migration on crime in counties with and without residency obligation. A negative coefficient would imply that crime is less affected by migration in counties with a residency obligation, while a positive coefficient implies the opposite. The coefficient of interest is in fact always negative but insignificant as well.

In contrast to crime, there is in fact a small positive and significant effect of the residence obligation on employment. In counties with the rule in place, one finds 12 additional employed migrants per 100,000 inhabitants. The determinants for migrant resettlement show that migrants actually settle in areas with more vacant jobs in states without a residency obligation [11] However, these jobs do not necessary be the best fit for the new migrants.

There are basically two ways migrants look for a job. Via networks or via the employment agency. In the case of main refugee counties, their population in Germany was rather low prior to 2014. Fig 5.2 shows their regional distribution in 2013 for all

[11] See table A4.

foreigners as well as potential labor force. One finds their maximum share of population was below 2 percent per county. Bell et al. (2013b) report that enclaves only play a role if they reach certain threshold in size. Another reason for limited effects of clustering is the fact that the number of migrants from the main refugee countries was small prior to 2015 and those who had arrived did not have time yet, to establish an efficient network (Beaman, 2012). However, A4 reveals that refugees in states with a residency obligation tend to be allocated in counties with higher share of pre-existing migrants from a similar background. Hence, network effects might actually play a larger role for migrants in states with a residency obligation. In addition, Brücker et al. (2016) find that 42 percent of all refugees in employment found their job via family and friends. This result is mostly driven by refugees with little or no formal education. Those with a formal education or university degree rather used the employment agency. Hence, especially for low skilled migrants, networks seem to play a role in terms of employment. As soon as micro-data on refugees is available, it is of interest to what extent labor market integration under both residency regimes differs by skill group.

5.6.2 Sensitivity

Two of the four states with a residency obligation in place decided at the end of 2016 or beginning 2017. Therefore, I run the analysis excluding Northrhine Westfalia. The results are shown in table A10. Excluding Northrhine Westfalia does not change the direction of results for employment. The coefficient becomes larger by 2 and is significant even on the 1 percent level. This reflects to some extent the better economic situation in Bavaria and Baden Wuertemberg compared to other German states, even though I control for unemployment.

Up to this point in time, the only control variable that is available for 2016 is the overall unemployment rate. Therefore, I carry out a sensitivity analysis by including the lagged covariates in the main DiD estimation. The variables included besides the unemployment rate are gross domestic product (GDP) and the share of young (< 25) in a county. Table A12 shows the results. One finds that the coefficients stay unaffected in terms of size and significance level. Hence, including unemployment suits as a valid proxy for overall differences in characteristics of states and counties.

In Gehrsitz and Ungerer (2016) we show that there has already been an increase in refugee numbers in 2014 and early 2015. Figure 5.1 in fact shows that there has been a significant increase in migrants in early 2015 and to some extent in 2014. Potential labor force from MRC only increased slightly in 2014. In order to rule out the possibility that my findings are affected by the choice of the relevant pre-treatment period, I use 2014 as the observation before intervention. Table A11 shows that the main result stays untouched. The effect on crime is within the same magnitude and still insignificant. Regarding employment of accepted refugees, I find slightly larger results on the same significance level. In fact, the coefficient DiD is 43.34 percent larger compared to the main analysis.

A potential threat to my empirical strategy is the possibility to capture unobserved differences in time trends in a DiD framework rather than a causal effect. In order to rule out such a scenario, I provide a placebo test in Table A13. For this test, I run the same analysis as in section 5.6 table A6 with 2014 as pre-treatment period and 2015 as post-treatment period. With such a specification, I do not find significant results for a causal effect of implementing a residency obligation on employment. In fact, the coefficient for the interaction effect DiD is more than three times smaller compared to the main findings. Hence, the placebo test provides further credibility to my findings.

5.7 Research Prospects

The research question I address in this paper is at the frontier of time. On the one hand, the question is highly relevant in the political context and from an academic point of view. On the other hand, a short term analysis potentially lacks the possibility of a long term conclusion, as the full consequences of the residency obligation are likely to be visible only after a certain time. Therefore, I include a section on potential research prospects with additional data and methods for the near future.

A first and natural improvement is the availability on data for the year 2017 (at least). This will allow for a more robust assessment of effects of the residency obligation on crime and employment. Moreover, it enables to make use of all four states with a residency obligation in place as the treatment group. In addition, micro data on refugees will become available in 2018.[12] Using these will be beneficial for two reasons. First of all, it enables researcher to actually identify accepted refugees compared to other migrants. Second, I will be able to draw conclusions not only on a county level but also on an individual basis.

One might be concerned with the validity of results in a DiD framework given heterogeneous characteristics of states prior to the introduction of a residency obligation.

[12] See Brücker et al. (2016) for first descriptive results.

As the policy was implemented on the state level, a comparison of aggregate measures is a potential robustness check of results. In order to construct a control group with similar characteristics the synthetic control group method first introduced to economic problems by Abadie et al. (2010) is a promising approach. However, for such an analysis, the full set of covariates predicting the outcome (employment and crime) and a sufficient time after the treatment period is needed.

Another way of tackling the potential problem of structural differences between control and treatment group is a combination of matching and DiD strategy. Heckman et al. (1997) provide a first application of this method to job training programs and provide a discussion of the methodology. Comparing counties in Bavaria and Baden Wuertemberg with counties in Saxony and Mecklenburg Western Pomerania might be subject to concerns, as these regions face remarkable differences in terms of economic situation. Therefore, the matching DiD approach intends to pair counties with similar characteristics from treatment and control group before running the actual DiD analysis. Similarly to the synthetic control group approach, a full set of county characteristics is required for this analysis.

Finally, a promising approach with recently increasing applications is the border discontinuity design (Lee and Lemieux, 2010). In order to apply this approach, a sufficiently large amount of observations on both sides of the boarder is needed. With data on four states there are 60 control counties and 40 treatment counties in a boarder discontinuity design. Hence, the full set of 2017 data on Lower Saxony and Northrhine Westfalia is needed.

5.8 Conclusion

The integration of accepted refugees continues to be an important topic in the light of increased migration all over Europe and in Germany in particular. Previously, migrants were observed to cluster in certain areas and form enclaves. The effect of this behavior is ambiguous from both, theoretical and empirical perspective. For Germany, there exists indeed some clustering of migrants, with potential negative effects on employment (Schaffner and Treude, 2014). First results on the recent wave of refugees indicate potential challenges in terms of crime and employment prospects (Gehrsitz and Ungerer, 2016).

As residency obligations have been argued to be successful in terms of labor market integration for ethnic Germans in the past, the German government installed an integration law. It requires accepted migrants to live in the the federal state in which their asylum claim was processed and in addition enables states to implement residency obligations on regional levels (e.g. county level). So far, Bavaria, Baden Wuertemberg, and Northrhine Westfalia make use of this rule, and Saxony Anhalt joined them in 2017.

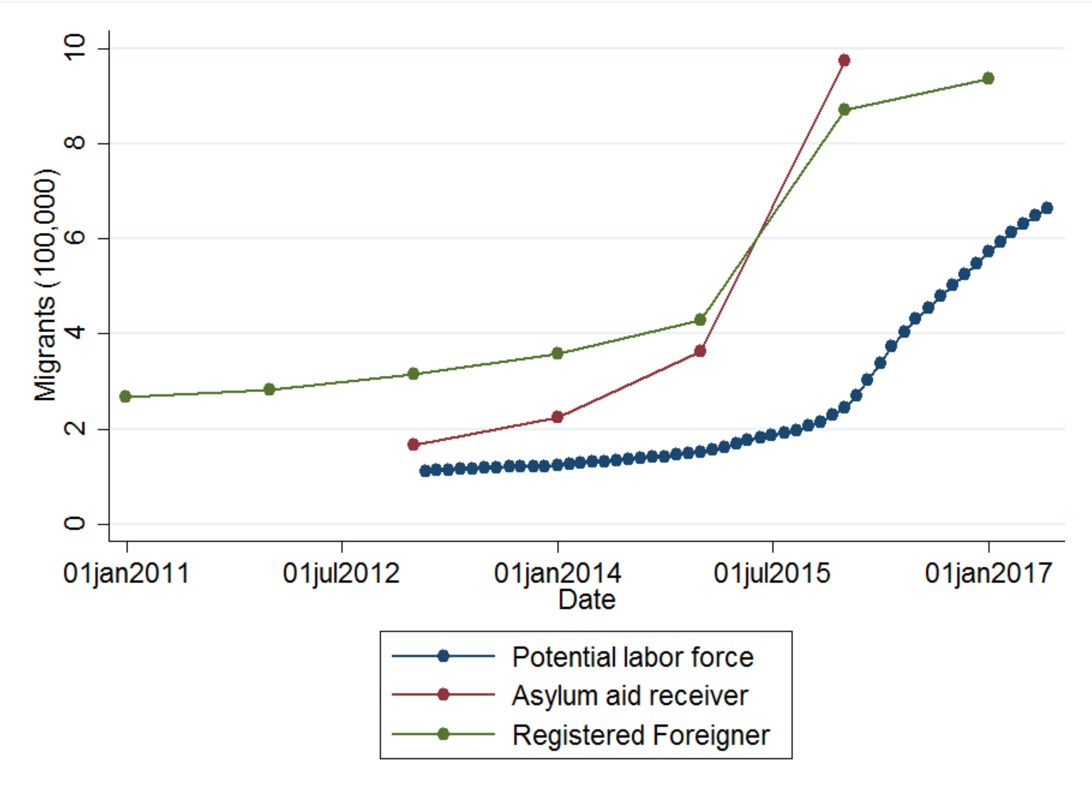
A first analysis of determinants for migrant settlement reveals substantial differences between counties in states with a residency obligation compared to those in states without such a rule. Assigned refugees in 2015 are a strong predictor for migrant resettlement in counties w/o residency obligation. Both schemes allocate more migrants in counties with higher unemployment. However, the effect is twice as large in counties with an residency obligation. Interestingly, migrants in states with a residency obligation are rather allocated in counties with higher shares of pre-existing immigrants from the main refugee counties. Hence, network effects might play a role in this context. In states without a residency obligation, accepted refugees tend to move to areas with more job vacancies.

The DiD analysis of the implementation of a residency obligation between 2015 and 2016 reveals mixed results for crime and employment. The effect for crime is negative, but insignificant. On the contrary, the residency obligation seems to help accepted refugees in terms of labor market integration. There are two potential channels at work. On the one hand, pre-existing clusters of migrants from similar background might serve as a network. Aggregate evidence on job search of refugees points in that direction (Brücker et al., 2016). To what extent this varies between states with and without residency obligation is, however, unclear, as micro-data is not yet available. On the other hand, job centers might be able to provide better service with a more even distribution of job seeker from refugee countries.

Due to the short time frame since implementation of the residency obligation, the effects I find are rather small and do not necessarily incorporate all channels at work. Therefore, a close monitoring of the development is highly interesting for continuing work on this topic. With micro data on refugees and additional crime data available in 2017 I will be able to draw more evidence on the effectiveness of a residency obligation for integration of accepted refugees.

Tables and Figures

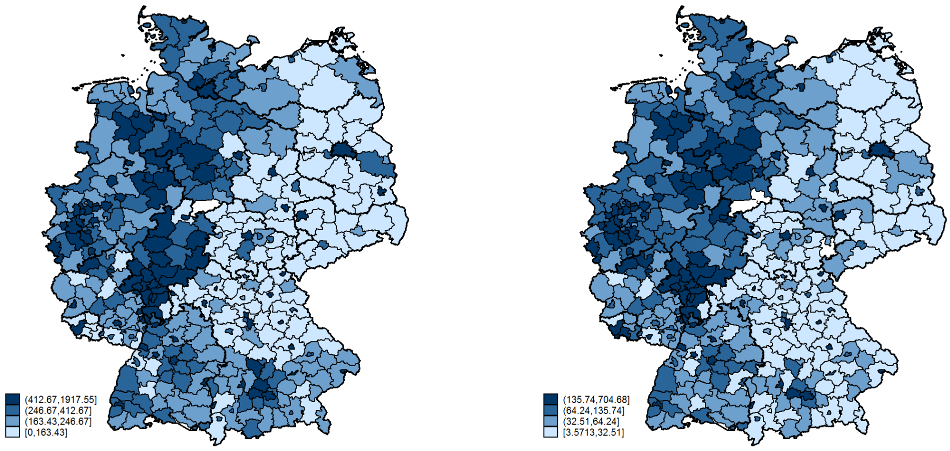
Figure 5.1 – Migration pattern using different data



Source: Federal Statistical Office (2017), Statistische Ämter des Bundes und der Länder (2017), Federal Employment Agency (2017a).
Notes: This graphs plots registered foreigners and potential labor force members from the 8 non-European refugee countries as well as all asylum aid receivers in 100,000.

Figure 5.2 – Distribution of migrants from main refugee counties in 2013

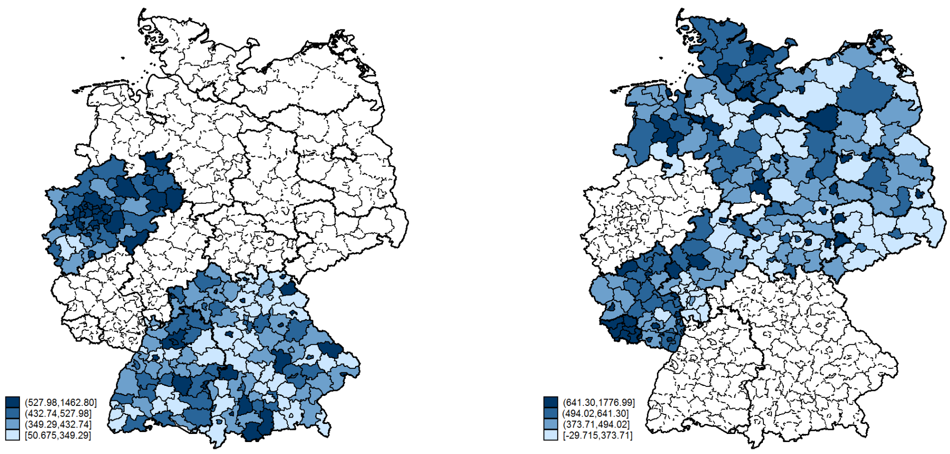
(a) All migrants   *(b)* Potential Labor force



Source: Federal Statistical Office (2017), Federal Employment Agency (2017a).
Notes: These maps show the regional distribution of registered foreigners and potential labor force members from the 8 non-European refugee countries in 2013.

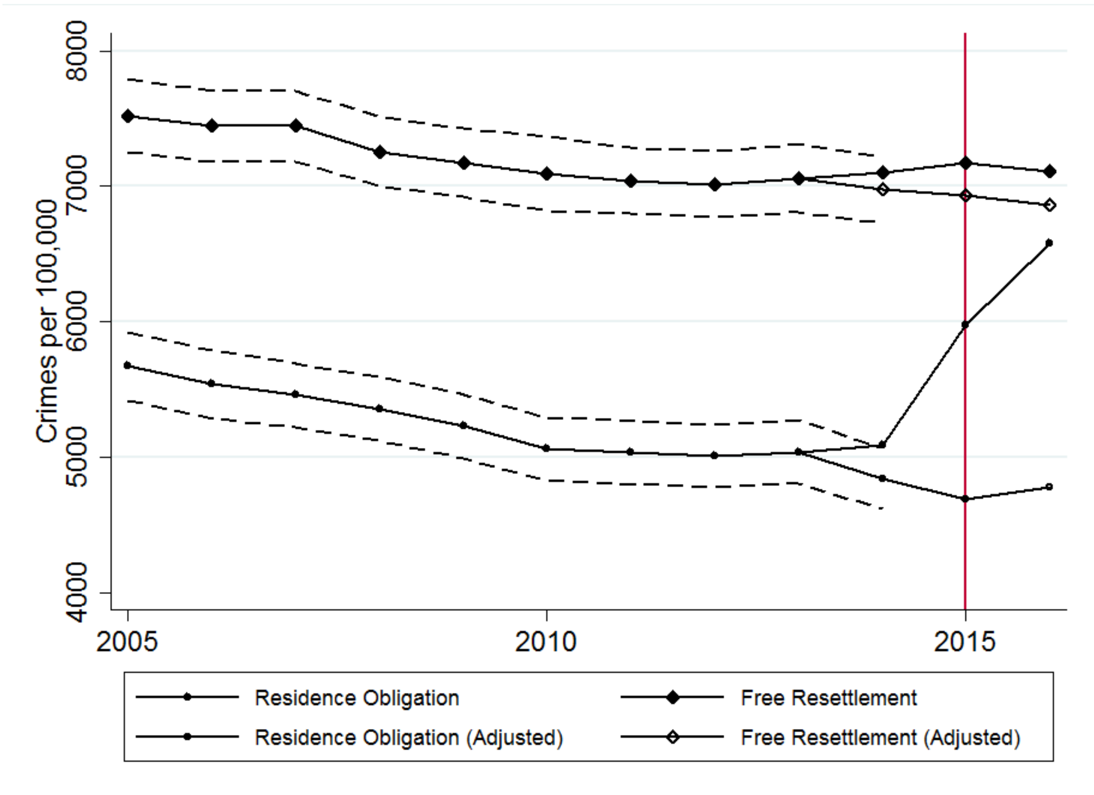
Figure 5.3 – Increase in Migrants between 2013 and 2016

(a) Residency obligation   *(b)* No residency obligation



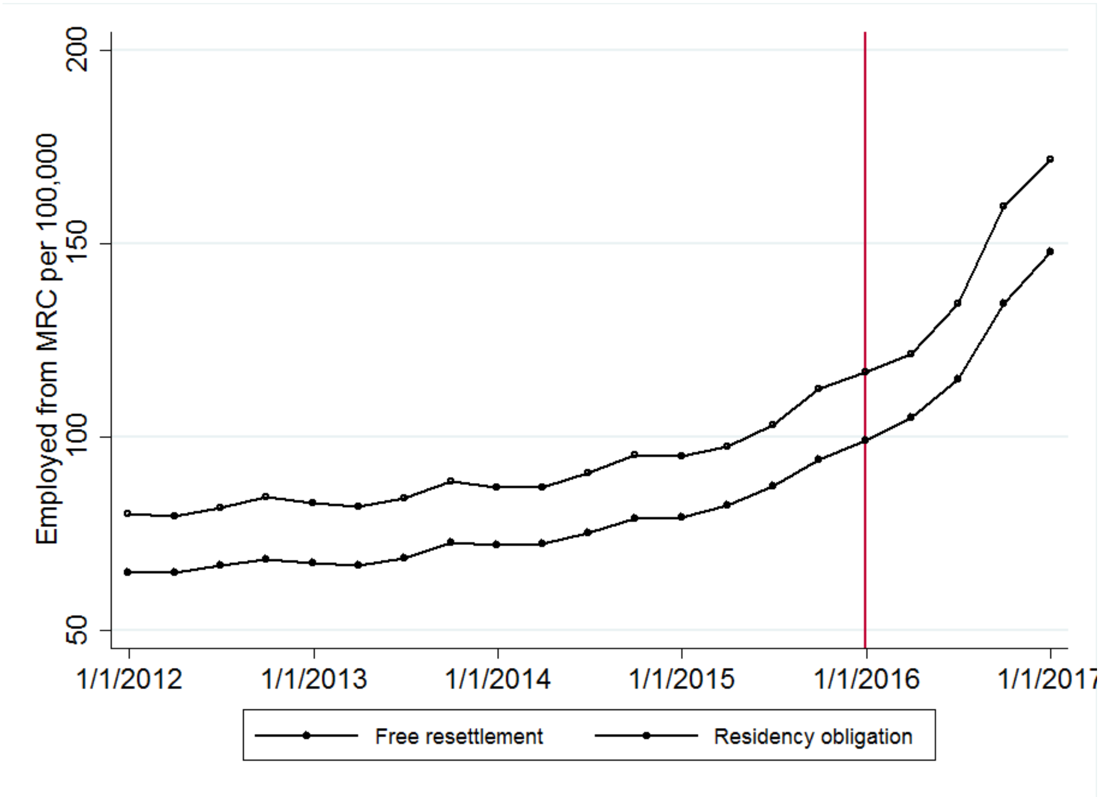
Source: Federal Statistical Office (2017), Federal Employment Agency (2017a).
Notes: These maps show the regional distribution of the difference in potential labor force members from the 8 non-European refugee countries between 2013 and 2016.

Figure 5.4 – Time trend for counties with and w/o residence obligation



Source: Federal Criminal Police Office (2017)
Notes: This figure shows annual crime rates (2005-2015) separately for states with and without residency obligation. Confidence intervals are at the 95 percent level.

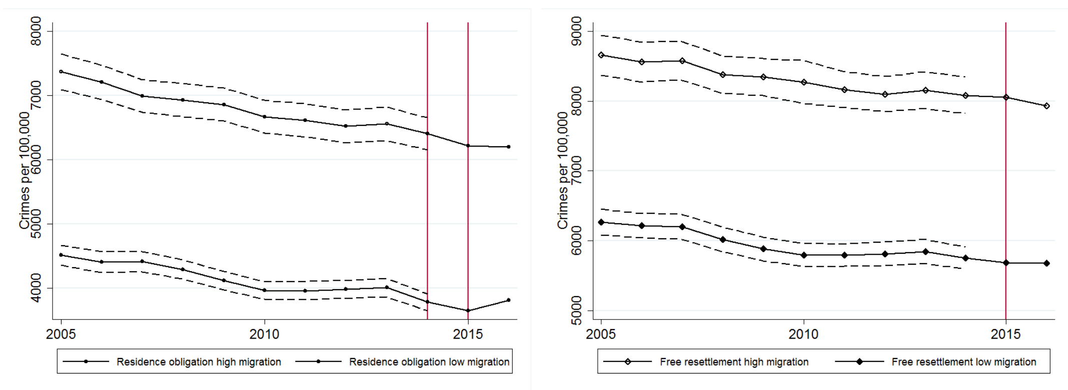
Figure 5.5 – Time trend for counties with and w/o residence obligation



Source: Federal Criminal Police Office (2017)
Notes: This figure shows quarterly employment figures from MRC separately for states with and without residency obligation. Confidence intervals are at the 95 percent level.

Figure 5.6 – Time trend for high and low migration within w and w/o residency obligation

(a) With residence obligation *(b)* Without residence obligation



Source: Federal Criminal Police Office (2017)
Notes: This figure shows annual crime rates (2005-2015) separately for states with and without residency obligation and high and low migration within these states. Confidence intervals are at the 95 percent level.

Table A1 – Residence Obligation by State

State	Residence Obligation	Decided	In Place
Baden-Wuertemberg	yes	16.08.2016	01.07.2016
Bavaria	yes	06.08.2016	01.01.2016
Berlin	-		
Brandenburg	no		
Bremen	-		
Hamburg	-		
Hesse	no		
Mecklenburg Western Pomerania	no		
Lower Saxony	no		
Northrhine-Westphalia (NRW)	yes	01.08.2016	01.12.2016
Rhineland Palatinate	no		
Saarland	no		
Saxony	no		
Saxony-Anhalt	yes	17.01.2017	17.02.2017
Schleswig-Holstein	no		
Thuringia	no		

Source: State Ministery of the Interior Bavaria (2017), State Ministery of the Interior Baden Wuertemberg (2016), State Ministery of the Interior Northrhine Westfalia (2016), State Ministery of the Interior Saxony Anhalt (2017).

Table A2 – High and low migration Inflows to counties

	All			Residence obligation			No Obligation		
	mean	med	sd	mean	med	sd	mean	med	sd
Absolut									
Migrants BA*	829	726	396	801	707	358	856	748	429
Migrants AZR**	634	583	416	646	629	439	622	509	394
Diff Migrants BA	601	554	274	570	538	234	629	571	305
Diff Migrants AZR	325	290	298	341	349	372	310	271	208
Migrants AZR 2011	198	129	211	204	134	209	193	122	213
Share Migration Background 2011	16.73	16.7	9.48	20.94	20.6	9.16	12.8	12.6	7.9

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a).
*federal employment agency (BA)
**register of foreigners (Ausländerzentralregister, AZR)

Table A3 – Summary Statistics

	All	No Obligation	Residence Obligation	Low Migration	High Migration
Migrants per 100,000 (potential labor force, BA)	652	696	604	507	797
Assigned refugees in 2015 per 100,000	848	916	775	765	933
Migrants per 100,000 (AZR)	634	622	647	495	771
Fully employed from MRC	131	114	148	103	158
Marginally employed from MRC	47	46	49	32	62
Unemployment rate 2015	5.76	6.81	4.62	5.37	6.15
Unemployment rate foreign	17.67	21.89	13.12	17.03	18.31
vacant jobs in 2015 per 100,000	725	714	738	729	723
GDP per capita in 2015	7020	6613	8587	3737	11373
Population in 2015	204417	194196	215485	118465	290766
Share young (¡25) in 2015)	15.07	12.57	18.77	17.18	18.28
# of schools per 100,000	42	43	41	44	40
# of hospital beds per 100,000	643	635	651	656	628
Housing Vacancies 2011	4.71	5.31	4.06	5	4
Average Age 2011	43.58	44	43	44	43
Share w. Migration Background 2011	16.74	13	21	13.48	20.03
Share Non-German 2011	6.24	4.71	7.90	4.84	7.67

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), Federal Employment Agency (2017b), federal statistical office.

Table A4 – Determinants of migrant resettlement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Migrants (AZR)		Migrants (BA)		Non employed (BA)		Employed (BA)	
Assigned refugees in 2015 per 100,000	0.12**	0.27***	0.47***	0.05*	0.48***	0.08**	0.01	-0.02***
	(0.05)	(0.07)	(0.05)	(0.03)	(0.06)	(0.03)	(0.01)	(0.01)
Reception center capacity 2015	0.05***	0.02	-0.01	-0.05***	-0.01	-0.05***	0.01**	-0.01**
	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)
Unemployment rate 2015	-3.62	12.72	14.49**	33.67***	24.75***	50.67***	-1.52	-2.58
	(6.45)	(19.07)	(5.93)	(8.35)	(7.02)	(9.27)	(1.37)	(1.91)
vacant jobs in 2015 per 100,000	0.05	0.12	0.18***	0.01	0.21***	0.01	0.02*	0.04***
	(0.06)	(0.13)	(0.05)	(0.06)	(0.06)	(0.06)	(0.01)	(0.01)
GDP per capita in 2015	-0.01**	0.00	0.00	0.00	0.01	-0.00	0.00	0.00***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Population in 2015	29.05**	-1.91	-29.68**	-16.40	-32.74**	-14.71	-3.10	-11.60***
	(14.23)	(44.04)	(13.10)	(19.28)	(15.50)	(21.41)	(3.03)	(4.42)
Urbanization	-25.26	-40.79	23.44	-16.11	28.17	-26.59	-0.05	-2.06
	(16.76)	(41.07)	(15.45)	(17.98)	(18.26)	(19.97)	(3.57)	(4.12)
Share young (25) in 2015	4,049.47***	3,321.31	9,000.09***	-1,830.78	11,458.09***	-2,033.93	161.62	599.04
	(1,313.13)	(5,029.72)	(1,213.10)	(2,201.52)	(1,430.56)	(2,445.63)	(279.69)	(504.38)
# of schools per 100,000 in 2015	1.26	5.44	-6.31***	1.21	-9.07***	1.49	-0.05	-0.03
	(1.67)	(3.46)	(1.54)	(1.52)	(1.81)	(1.68)	(0.35)	(0.35)
# of hospital beds per 100,000 in 2015	0.01	0.06	0.01	0.06	0.05	0.05	0.03**	0.01
	(0.05)	(0.08)	(0.04)	(0.04)	(0.05)	(0.04)	(0.01)	(0.01)
Housing Vacancies 2011	-3.74	-56.20*	26.05***	29.91**	25.32***	31.46**	-2.12	-10.63***
	(8.13)	(31.07)	(7.53)	(13.60)	(8.85)	(15.11)	(1.73)	(3.12)
Average Age 2011	45.32**	63.65	43.47***	-12.66	54.01***	-15.92	2.69	6.37
	(17.65)	(50.46)	(16.40)	(22.08)	(19.23)	(24.53)	(3.76)	(5.06)
Share w. Migration Background 2011	1.81	9.97*	-0.68	1.65	-3.37	1.99	0.77	0.47
	(2.76)	(5.79)	(2.54)	(2.53)	(3.01)	(2.82)	(0.59)	(0.58)
Migrants from refugee countries in 2011	0.45***	0.03	-0.05	0.21***	-0.09	0.22**	0.03	0.04**
	(0.09)	(0.18)	(0.09)	(0.08)	(0.10)	(0.09)	(0.02)	(0.02)
Observations	208	193	207	193	208	193	208	193
R-squared	0.36	0.26	0.61	0.45	0.61	0.51	0.35	0.45
Residency Obligation	No	Yes	No	Yes	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office, Gehrsitz and Ungerer (2016).
∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A5 – Crime and number of migrants

	Migrants (BA)		Migrants (AZR)		Migrants (Asylum Benefit	
	Overall Crime	Overall Crime	Overall Crime	Overall Crime	Overall Crime	Overall Crime
	(1)	(2)	(3)	(4)	(5)	(6)
# of migrants/pop	2.8031***	1.9269***	0.8093*	0.5033	0.4909*	0.3632
	(0.5649)	(0.6144)	(0.4290)	(0.4992)	(0.2947)	(0.2928)
Residency Obligation		-693.2592		-594.8822		-537.4015
		(436.2744)		(436.9196)		(445.1895)
Unemployment Rate		311.2348***		421.8859***		450.8979***
		(94.7761)		(95.3998)		(89.9558)
Observations	400	400	401	400	399	398
R-squared	0.0685	0.1137	0.0153	0.0930	0.0104	0.0973
Covariates	No	Yes	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.
$*{*}*/{*}*/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A6 – Effect of residency obligation on crime and employment

	Overall Crime		Employed from Refugee Countries	
	(1)	(2)	(1)	(2)
D	-30.3203	120.9036	41.6741***	41.6579***
	(59.4185)	(110.5570)	(4.8789)	(4.9934)
Residency Obligation (RO)	-1,493.9727	-539.7303	21.8015	21.6985
	(1,019.1796)	(1,723.8384)	(14.5806)	(16.2205)
DiD	70.1966	-15.6122	12.2169*	12.2261*
	(87.9087)	(96.3502)	(6.2153)	(6.0717)
Observations	800	800	802	802
R-squared	0.0292	0.0875	0.0978	0.0978
Covariates	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.

$***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A7 – Differential effect of migration on crime

	Migrants per 100,000 (BA)		Migrants per 100,000 (AZR)	
	(1)	(2)	(3)	(4)
D	-1,603.9799***	-1,202.6669**	-390.1603	-22.5871
	(466.1859)	(512.4936)	(502.4622)	(479.3618)
Residency Obligation (RO)	-2,448.3968***	-1,023.8761	-2,934.9915***	-1,104.0578
	(819.2946)	(953.3440)	(934.2665)	(1,097.4340)
Migrants (BA)	0.5966	0.3473	-0.6413	-0.4676
	(1.4449)	(1.3968)	(0.9654)	(0.9242)
RO× D	-72.3434	30.7787	-219.2079	-685.0972
	(920.6842)	(1,096.1993)	(646.3050)	(475.6142)
Migrants × RO	2.8037	0.8839	3.5453	1.4281
	(3.2084)	(2.8754)	(2.9006)	(2.3196)
Migrants × D	1.5025*	1.3159*	0.6445	0.4401
	(0.7465)	(0.6342)	(0.6049)	(0.5655)
DiDiD	-1.2920	-0.4377	-1.8514	-0.2986
	(0.8935)	(0.6116)	(2.0502)	(1.4307)
Observations	799	799	800	800
R-squared	0.0710	0.1010	0.0519	0.0947
Covariates	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.
$***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A8 – Results for selected crime subgroups

	Assault		Drug		Property		Street		Violence	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
D	35.1215***	51.4152***	32.4245***	40.7287**	-2.5043	29.6854	-73.3015***	-5.2076	11.0150***	21.4634***
	(9.4354)	(17.4351)	(8.3413)	(14.3469)	(6.0407)	(23.9964)	(11.1211)	(34.3260)	(3.4353)	(6.9519)
Residency	-35.9902	21.8542	-43.7545	-14.2735	-156.0494	-41.7726	-308.8805	-67.1397	-40.3481	-3.2550
Obligation	(126.8266)	(186.5006)	(90.0815)	(131.0010)	(146.3655)	(235.9425)	(185.2816)	(316.4710)	(35.6059)	(62.5274)
DiD	-8.0640	-16.8536	8.3043	3.8245	-21.5523	-38.9172	16.6557	-20.0778	0.6365	-5.0000
	(11.4112)	(11.3032)	(24.4965)	(20.6592)	(17.9387)	(27.1588)	(33.9865)	(49.0600)	(5.2528)	(5.7518)
Observations	800	800	800	800	800	800	800	800	800	800
R-squared	0.0038	0.0238	0.0063	0.0141	0.0231	0.0674	0.0308	0.1096	0.0180	0.0773
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.
$***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

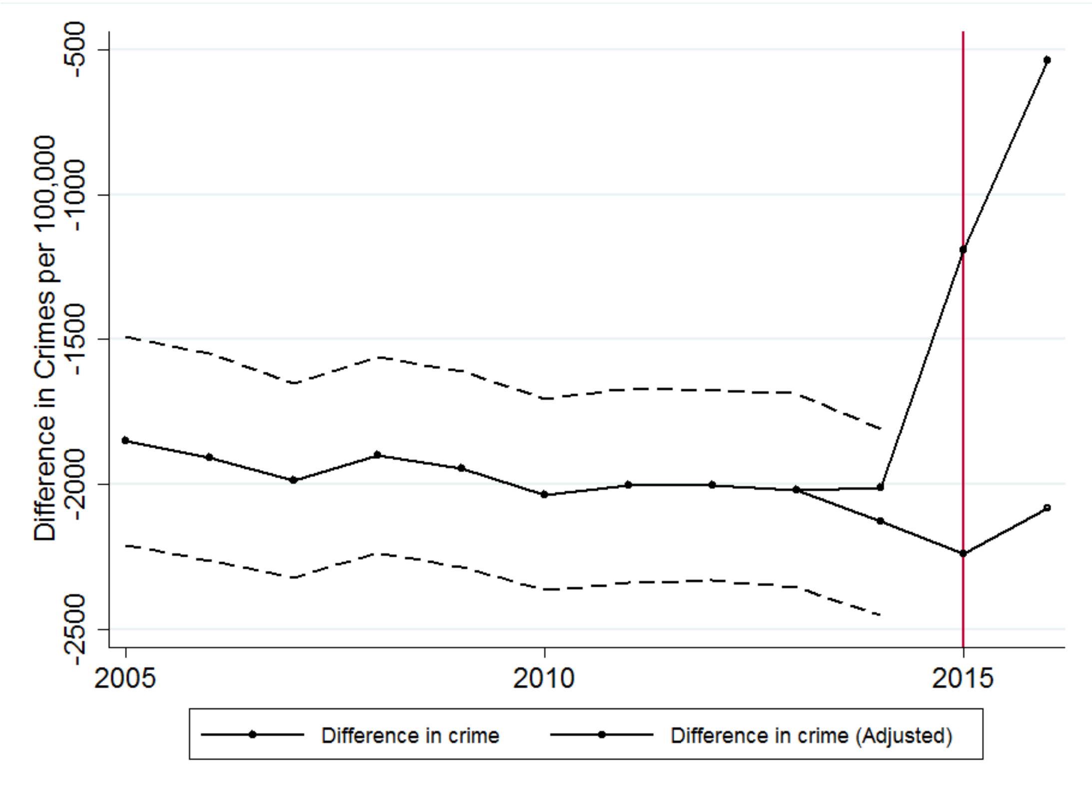
Table A9 – Differential effect of migration on Employment

	Employed refugee context					
	Migrants (AZR)		Migrants (BA)		Potential labor force	
	(1)	(2)	(3)	(4)	(5)	(6)
D	23.3172	6.6602	23.3172**	6.6602	29.7975	13.7030
	(15.4486)	(14.4133)	(11.6852)	(10.5869)	(18.1018)	(17.0086)
Residency Obligation (RO)	25.6497***	-8.8496	25.6497***	-8.8496	28.5436***	-3.4411
	(6.3108)	(11.2779)	(7.1049)	(8.1001)	(6.3963)	(10.4855)
Migrants (BA)	0.3267***	0.3269***	0.3267***	0.3269***	0.3707***	0.3721***
	(0.0468)	(0.0441)	(0.0406)	(0.0398)	(0.0551)	(0.0512)
RO × D	27.8430	25.7673	27.8430	25.7673	30.6868	29.5483
	(17.2048)	(15.6479)	(17.3158)	(16.7031)	(19.0306)	(17.9471)
Migrants× RO	-0.0312	0.0601	-0.0312	0.0601	-0.0508	0.0494
	(0.0984)	(0.0876)	(0.0570)	(0.0560)	(0.1122)	(0.0986)
Migrants× D	-0.2044***	-0.1897***	-0.2044***	-0.1897***	-0.2518***	-0.2374***
	(0.0359)	(0.0318)	(0.0376)	(0.0365)	(0.0474)	(0.0421)
DiDiD	0.0179	-0.0414	0.0179	-0.0414	0.0273	-0.0420
	(0.0592)	(0.0583)	(0.0524)	(0.0520)	(0.0766)	(0.0728)
Observations	802	802	802	802	802	802
R-squared	0.4232	0.4640	0.4232	0.4640	0.3870	0.4219
Covariates	No	Yes	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.
$***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

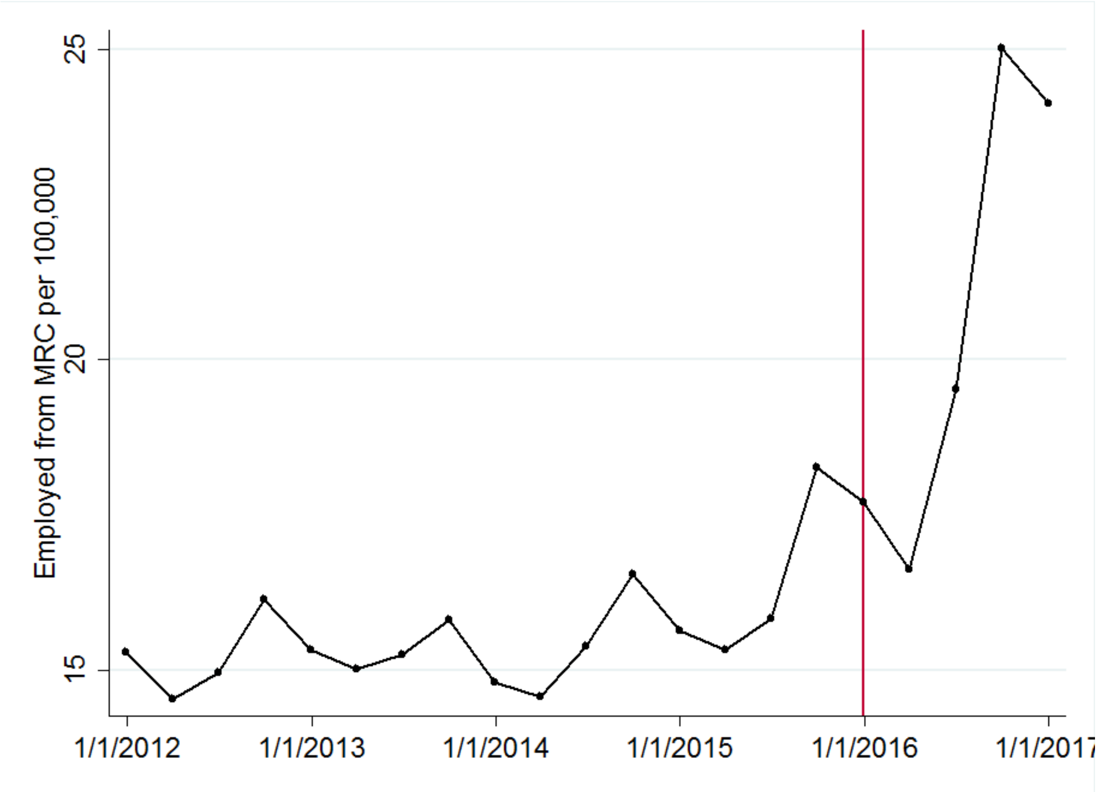
Appendix: Integrating Refugees: How Effective is a Residency Obligation

Figure 7 – Difference in time trend for counties with and w/o residence obligation



Source: Federal Criminal Police Office (2017)
Notes: This figure shows the difference in annual crime rates (2005-2015) for states with and without residency obligation Confidence intervals are at the 95 percent level.

Figure 8 – Difference in time trend for counties with and w/o residence obligation



Source: Federal Criminal Police Office (2017)

Notes: This figure shows the difference in quarterly employment from MRC for states with and without residency obligation Confidence intervals are at the 95 percent level.

Table A10 – Effect of residency obligation on integration excluding Northrhine Westfalia

	Overall Crime		Employed from Refugee Countries	
	(1)	(2)	(1)	(2)
D	-108.4134**	322.0668	58.5043***	59.5910***
	(40.0747)	(190.8368)	(5.1977)	(5.4914)
Residency Obligation	-346.5467	2,183.6062	15.4283	21.8259
	(1,106.2370)	(1,662.9642)	(11.2071)	(16.0446)
DiD	-1.8722	-240.5215	23.5182***	22.9174***
	(57.3038)	(145.6953)	(5.9860)	(5.9857)
Observations	800	800	802	802
R-squared	0.0016	0.1172	0.1686	0.1704
Covariates	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.

$***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A11 – Effect of residency obligation on integration with pre-treatment period 2014

	Overall Crime		Employed from Refugee Countries	
	(1)	(2)	(1)	(2)
D	-107.2380**	183.9795	58.3847***	58.4317***
	(50.2333)	(164.6534)	(6.5204)	(6.8189)
Residency Obligation	-1,419.9818	-386.1284	16.7099	16.8772
	(1,074.3440)	(1,827.5447)	(12.5638)	(13.6578)
DiD	-3.7942	-160.8923	17.3084*	17.2831*
	(58.2918)	(117.7403)	(9.3777)	(9.1126)
Observations	800	800	802	802
R-squared	0.0275	0.0873	0.1661	0.1661
Covariates	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.

$*{*}*/{*}*/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A12 – Effect of residency obligation on integration with lagged covariates

	Overall Crime		Employed from Refugee Countries	
	(1)	(2)	(1)	(2)
D	-30.3203	173.9235	41.6741***	40.9150***
	(59.4185)	(109.4779)	(4.8789)	(4.6940)
Residency Obligation	-1,493.9727	-286.1034	21.8015	-13.3805
	(1,019.1796)	(1,649.2257)	(14.5806)	(20.8872)
DiD	70.1966	-20.6579	12.2169*	12.0968*
	(87.9087)	(97.1421)	(6.2153)	(6.3238)
Observations	800	800	802	802
R-squared	0.0292	0.1711	0.0978	0.4176
Covariates	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.
∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

Table A13 – Placebo test for DiD using 2014/2015

	Overall Crime		Employed from Refugee Countries	
	(1)	(2)	(1)	(2)
D	-76.9177	55.8883	16.7105***	16.7725***
	(51.5139)	(80.9271)	(1.9857)	(2.1808)
Residency Obligation	-1,419.9818	-428.9502	16.7099	17.1729
	(1,074.3440)	(1,774.4260)	(12.5638)	(13.1288)
DiD	-73.9908	-141.5391	5.0916	5.0601
	(103.4960)	(114.2585)	(3.5120)	(3.4216)
Observations	800	800	802	802
R-squared	0.0287	0.0877	0.0389	0.0389
Covariates	No	Yes	No	Yes

Source: Federal Statistical Office (2017), Federal Employment Agency (2017a), federal statistical office.
$***/**/*$ indicate significance at the 1%/5%/10%-level. Standard errors clustered at the state level.

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Curriculum Vitae

Employment

04/2016 -	Researcher Research Group International Distribution and Redistribution Centre for European Economic Research (ZEW), Mannheim
04/2014 - 03/2016	Scientific Assistant of the President, Researcher Research Group International Distribution and Redistribution Centre for European Economic Research (ZEW), Mannheim

Education

04/2014 –	Doctoral Studies in Economics, University of Cologne (Supervisor: David Jaeger)
10/2010 - 12/13	Master of Science Economics, University of Cologne (Supervisor: David Jaeger)
01/2012 - 05/12	Exchange Semester Norwegian School of Economics, NHH Bergen
04/2007 – 09/10	Bachelor of Science Economics and Business Administration Goethe-University Frankfurt am Main
09/1997 – 06/2006	*Abitur* (High School) Woehlergymnasium, Frankfurt am Main

Research Visit

03/2016 - 05/2016	National Bureau of Economic Research (NBER), New York